

VII. *An Enquiry concerning the Nature of Heat, and the Mode of its Communication.* By Benjamin Count of Rumford, V. P. R. S. Foreign Associate of the National Institute of France, &c.

Read February 2, 1804.

HEAT is employed in such a vast variety of different processes, in the affairs of life, that every new discovery relative to it must necessarily be of real importance to mankind; for, by obtaining a more intimate knowledge of its nature and mode of action, we shall no doubt be enabled not only to excite it with greater economy, but also to confine it with greater facility, and direct its operations with more precision and effect.

Having many years ago found reason to conclude, that a careful observation of the phenomena which attend the heating and cooling of bodies, or the communication of heat from one body to another, would afford the best chance of acquiring a farther insight into the nature of heat, my view, in all my researches on this subject, has been principally directed to that point; and the experiments of which I am now to give an account, may be considered as a continuation of those I have already, at different times, had the honour of laying before the Royal Society, and of presenting to the public in my Essays.

In order that the attention of the Society may not be interrupted unnecessarily, by descriptions of instruments, in the midst of the accounts of interesting experiments, I shall begin by describing the apparatus which was provided for these researches;

and, as a perfect knowledge of the instruments made use of, is indispensably necessary, in order to form distinct ideas of the experiments, I shall take the liberty to be very particular in these descriptions.

The thermometers, four in number, which were used in these experiments, were constructed under my own eye, and with the greatest possible care; and, after every trial I have been able to make with them, in order to ascertain their accuracy, they appear to be very perfect.

They are mercurial thermometers, graduated according to FAHRENHEIT: their bulbs are cylindrical, 4 inches long, and $\frac{4}{10}$ of an inch in diameter; and their tubes are from 15 to 16 inches long. The mercury with which they are filled is quite pure; and they are freed from air. Their scales were divided with the greatest care; and, by means of a nonius, they show eighth parts of a degree very distinctly: they are graduated from about 10 degrees below the freezing point, to 5 or 6 degrees above the point of boiling water. Their bulbs are quite naked; their scales ending about 1 inch above the junction of the bulb with its tube. The freezing point is situated about 5 inches above the upper end of the bulb. The reason for placing it so high, will be evident, from the details of the experiments in which these instruments were used.

The instrument I contrived for ascertaining the warmth of clothing, is extremely simple: it is merely a hollow cylindrical vessel, made of thin sheet brass. It is closed at both ends; and has a narrow cylindrical neck, by which it is occasionally filled with hot water.

This vessel, being covered with a garment made to fit it,

composed of any kind of cloth, or stuff, or other warm covering, is supported, in a vertical position, on a wooden stand, which is placed on a table, in a large quiet room; and, one of the thermometers above described being placed in the axis of the vessel, the time employed in cooling the water, through the clothing with which the instrument is covered, is observed and noted down.

Now, as the time of cooling through any given interval of the scale of the thermometer, (or from any given degree above the temperature of the air of the room, to any other given lower degree, but still above the temperature of the air of the room,) will be longer, or shorter, as the covering of the instrument is more or less adapted for confining heat, it is evident, that the relative warmth of clothing of different kinds, may be very accurately determined by experiments of this sort.

I provided four instruments of this kind, all very nearly of the same dimensions. Their cylindrical bodies are each 4 inches in diameter, and 4 inches long; and their cylindrical necks are about $\frac{8}{10}$ of an inch in diameter, and 4 inches in length. This neck is placed in the centre of the circular flat top, or upper end, of the vertical cylindrical body; and, opposite to it, in the centre of the flat bottom of the body, there is a hollow cylinder, $\frac{8}{10}$ of an inch in diameter, and 3 inches long, projecting downwards, into which a vertical cylinder of wood is fitted, on the top of which the instrument is supported, in such a manner that the air has free access to every part of it. This cylinder of wood constitutes a part of the wooden stand above-mentioned.

As the thermometer is placed in the axis of the cylindrical vessel, and as its bulb is just as long as the body of this vessel, it is evident that it must ever indicate the *mean temperature* of

the water in the vessel, however different the temperature of that water may be at different depths.

The thermometer is firmly supported in its place, by causing a part of the lower end of its scale to enter the neck of the cylindrical vessel, and to fit it with some degree of accuracy, but not so nicely as to be in danger of sticking fast in it.

The lower end of the bulb of the thermometer does not absolutely touch the bottom of the vessel, but it is very near touching it.

Figure. 1 (Plate IV.) will give a clear idea of this instrument, placed on its wooden stand, which is so contrived, that the instrument may be placed higher, or lower, at pleasure.

The foregoing description of this instrument is so particular, that the figure will be easily understood, without any farther illustration. The cylindrical vessel is represented placed on the stand, with its thermometer in its place.

As, in some of the first experiments I made with this instrument, I found it difficult to apply the coverings which I used, to the ends of the body of the instrument, I endeavoured, by covering up those ends with a permanent and very warm covering, to oblige most of the heat to pass off through the vertical sides of the instrument; to which it was easy to fit almost any kind of covering, and more especially coverings of various thicknesses of confined air, the relative warmth of which I was very desirous of ascertaining.

The means I employed for covering up the ends of the instrument were as follows. Having provided two thin cylindrical wooden boxes, (like common pill-boxes, but much larger,) something less in diameter than the body of the instrument, and $2\frac{1}{2}$ inches deep, I dried them as much as possible; and, after

having varnished them, within and without, with spirit varnish, I covered them, within and without, with fine wove writing-paper, and then gave the paper three coats of the same varnish. I then perforated the bottoms of these boxes with round holes, just large enough to admit the neck of the instrument, and the cylindrical projection at its bottom; and then inverted them over the two ends of the instrument, filling the boxes at the same time with *eider-down*.

These boxes were fixed and confined in their places, by means easy to be imagined; and, in order to confine the heat still more effectually, each of the boxes was covered on the outside with a cap of fur, as often as the instrument was used; as was also that part of the neck of the instrument which projected above the box.

Two of the instruments, which I shall distinguish by the numbers 1 and 2, were covered up at their ends in this manner: the other two instruments, No. 3 and No. 4, were left in the state represented by the Figure 1; that is to say, the ends of their cylindrical bodies were not covered with permanent coverings.

In each experiment, two similar instruments (No. 1 and No. 2, for instance, or No. 3 and No. 4) were used, the one *naked*, and the other *covered*; and, as the naked instrument always served as a standard, with which the results of the experiments made with the other were compared, it is evident, that this arrangement rendered the general results of the experiments much more satisfactory and conclusive than they could possibly have been, had the experiments made on different days, and with various kinds of covering, been made singly, or unaccompanied by a fixed and invariable standard.

The experiments were made, and registered, in the following

manner: the two instruments used in the experiment, placed on their wooden stands, being set down on the floor, were filled to within about $1\frac{1}{2}$ inch of the tops of their cylindrical necks with boiling hot water; and, a thermometer being put into each of them, they were placed, at the distance of 3 feet from each other, on a large table, in a corner of a large quiet room,* where they were suffered to cool, undisturbed. Near them, on the same table, and at the same height above the table, there was placed another thermometer, (suspended in the air, to the arm of a stand,) by which the temperature of the air of the room was ascertained from time to time.

No person was permitted to pass through the room, while an experiment was going on; and, in order to prevent, as far as it was possible, all those currents of air in the room which were occasioned by partial heat, produced by the light which came in at the windows, the window-shutters were kept constantly shut; one of them only being opened for a moment, now and then, just to observe the thermometers, and note down the progress of the experiment.

The results of each experiment were entered on a separate sheet of paper; which paper was previously prepared for that use, by being divided into separate vertical columns, by lines drawn with a pen, and ruled in parallel horizontal lines with a lead pencil.

The following is an exact copy of one of these register-sheets; and contains the results of an actual and very interesting experiment, which lasted 26 hours.

* This room, which is adjoining to my laboratory, in my house at Munich, is 19 feet wide, 24 feet long, and 13 feet high.

“ Experiments on Heat, made at Munich, 11th March, 1803.
 “ The large cylindrical Vessels, No. 1 and No. 2, (made of thin
 “ sheet brass,) were filled with hot Water, and exposed to
 “ cool in the Air of a large quiet Room. The Ends of both
 “ these Instruments were well covered with warm Clothing,
 “ Furs, &c. The vertical polished Sides of No. 1 were *naked*.
 “ The Sides of No. 2 were *covered* with one Thickness of fine
 “ white *Irish Linen*, which had been worn, strained over the
 “ metallic Surface.”

Time.		Temperature		Tem- perature of the air.	Time.		Temperature		Tem- perature of the air.
h.	min.	of No. 1, <i>naked</i> .	of No. 2, <i>covered</i> .		h.	min.	of No. 1, <i>naked</i> .	of No. 2, <i>covered</i> .	
10	10	126 $\frac{1}{2}^{\circ}$	126 $^{\circ}$	43 $\frac{1}{4}^{\circ}$	4	—	61 $\frac{3}{4}^{\circ}$	53 $\frac{1}{2}^{\circ}$	43 $\frac{1}{2}^{\circ}$
—	30	109 $\frac{1}{2}$	106 $\frac{1}{2}$	43 $\frac{1}{2}$	—	30	59 $\frac{1}{2}$	52	—
—	45	105	100 $\frac{1}{8}$	43 $\frac{3}{4}$	5	30	57	49 $\frac{3}{4}$	42 $\frac{1}{2}$
11	—	101 $\frac{1}{4}$	94 $\frac{3}{4}$	44	6	—	55 $\frac{1}{2}$	49 $\frac{1}{8}$	—
—	2 $\frac{1}{2}$	—	94	—	—	30	54 $\frac{1}{4}$	48 $\frac{1}{4}$	—
—	15	97 $\frac{1}{2}$	90 $\frac{1}{4}$	—	7	—	53 $\frac{1}{2}$	47 $\frac{1}{2}$	42
—	30	94	86 $\frac{1}{4}$	—	8	—	51 $\frac{1}{2}$	46 $\frac{1}{2}$	—
—	39	—	84	—	9	—	50	45 $\frac{3}{4}$	—
—	45	91 $\frac{1}{4}$	82 $\frac{1}{2}$	—	10	—	49	45	—
12	—	88 $\frac{1}{2}$	79 $\frac{3}{8}$	—	8	12th Mar.	43	42	40
—	15	85 $\frac{1}{2}$	76	—	The instruments were now removed into a warm room.				
—	25	84	—	—	8	2	43	42	62
—	30	—	74 $\frac{1}{2}$	—	—	32	44 $\frac{3}{4}$	44 $\frac{3}{4}$	62 $\frac{1}{2}$
—	45	80	70	—	—	47	46	46 $\frac{1}{2}$	63
1	—	78	68 $\frac{1}{8}$	—	9	24	48	49 $\frac{1}{2}$	—
—	30	74 $\frac{1}{4}$	64 $\frac{1}{4}$	—	10	—	50	52	—
2	—	71 $\frac{1}{8}$	61 $\frac{1}{2}$	43 $\frac{3}{4}$	—	41	51 $\frac{1}{2}$	53 $\frac{7}{8}$	—
—	30	68 $\frac{1}{8}$	58 $\frac{3}{4}$	43 $\frac{1}{2}$	12	—	54	56 $\frac{1}{2}$	—
3	—	65 $\frac{3}{4}$	56 $\frac{3}{4}$	—	12	26	54 $\frac{1}{2}$	57	—
—	30	63 $\frac{1}{2}$	54 $\frac{3}{4}$	—	An end was now put to the experiment.				

Though it was easy to discover, by a single glance at the register, whether a covering which was put over one of the instruments prolonged the time of its cooling or not; yet, in order to compare the results of different experiments, and particularly of such as were made on different days, so as to determine with precision *how much* warmer one kind of covering was than another, it was necessary to fix on some particular interval in the scale of the thermometer, or number of degrees, commencing at some certain invariable number of degrees above the temperature of the air by which the instrument was surrounded, in order that the warmth of the covering, or its power of confining heat, might with certainty be estimated by the time employed in cooling through that interval.

By the results of a great number of experiments I found, that the same instrument cooled through any given (small) number of degrees, (10 degrees, for instance,) in very nearly the same time, whatever was the temperature of the air of the room; provided always, that the point from which these 10 degrees commenced, was at the same given number of degrees above the temperature of the air at the time being.

The interval I chose for comparing the results of my experiments, is that which commences with the *fiftieth*, and ends with the *fortieth* degree of FAHRENHEIT'S thermometer, *above the temperature of the air in which the instrument is exposed to cool*. When, for instance, the air was at 58° , the interval commenced at the 108th degree, and ended at the 98th. When the air was at $64\frac{1}{2}^{\circ}$, it commenced at $114\frac{1}{2}^{\circ}$, and ended at $104\frac{1}{2}^{\circ}$.

That the same instrument, exposed to cool in the air, does in fact cool the same number of degrees in the same time, very nearly, when the given interval of the scale of the thermometer

is reckoned from the same height, or given number of degrees above the temperature of the air at the time when the experiment is made, will appear from the following results of 11 different experiments, made on different days, and when the air in which the instrument was exposed to cool was at different degrees of temperature.

The large cylindrical vessel, No. 1, having its two ends well covered up with eider-down, furs, &c. its vertical sides being exposed *naked* to the air, in a large quiet room, was found to cool 10 degrees, *viz.* from the 50th to the 40th degree above the temperature of the air in which it was exposed, as follows.

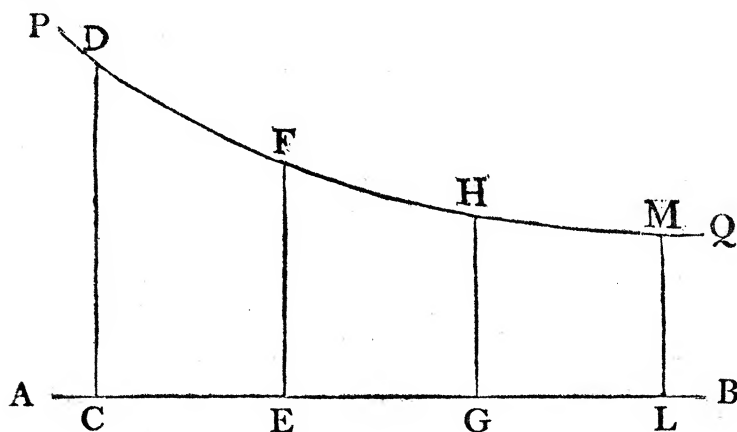
Temperature of the air.		Degrees cooled.		Time employed in cooling.
44°	- -	from 94° to 84°	- -	55 minutes.
45 $\frac{1}{4}$	- -	95 $\frac{1}{4}$ to 85 $\frac{1}{4}$	-	55 $\frac{1}{2}$
48	- -	98 to 88	- -	55 $\frac{1}{4}$
51 $\frac{1}{2}$	- -	101 $\frac{1}{2}$ to 91 $\frac{1}{2}$	- -	55 $\frac{1}{2}$
52	- -	102 to 92	- -	55
54	- -	104 to 94	- -	54 $\frac{1}{4}$
44	- -	94 to 84	- -	55 $\frac{5}{6}$
42 $\frac{1}{2}$	- -	92 $\frac{1}{2}$ to 82 $\frac{1}{2}$	- -	55 $\frac{1}{3}$
45	- -	95 to 85	- -	56
46	- -	96 to 86	- -	55
44	- -	94 to 84	- -	55 $\frac{1}{3}$

The fact which these experiments are here brought to prove, has likewise been confirmed by other experiments, made with other instruments, and at times when the temperature of the air has been as high as 64°; but I will not take up the time of the Society, by giving a particular account of them in this place.

As it sometimes happened, though very seldom, in the course

of an experiment, (which commonly lasted several hours,) that I was called away, and was not present to observe the thermometer, at the moment of the passage of the mercury through one or both of those points of its scale which formed the limits of the given interval, chosen as the standard for a comparison of the results of the experiments with each other, it became a matter of considerable importance, to find means for supplying these accidental defects, and ascertaining the points in question by interpolation.

In order to facilitate the means of doing this, I endeavoured to investigate the law of the cooling of hot bodies in a cold fluid medium; and I found reason to conclude,



That if, on the right line AB, a perpendicular CD be taken, equal to the difference of the temperatures of the hot body and of the colder medium, expressed in degrees of the thermometer; and, after a certain given time, represented by CE, taken on the line AB, at the point E, another perpendicular EF be erected, and EF be taken equal to the difference of the temperatures after the time represented by CE has elapsed; and if the

perpendiculars GH and LM be drawn, representing the difference of the temperatures after the times EG and GL have elapsed, a curved line PQ, drawn through the points D, F, H, M, will be the logarithmic curve; or, if it vary from that curve, its variation, within the limits answering to a change of temperature amounting to a few degrees, (especially if they be taken when the temperature of the hot body is about 40 or 50 degrees above that of the medium,) will be so very small, that no sensible error will result from a supposition that it is the logarithmic curve, in supplying, by computation, any intermediate observations, which happen to have been neglected in making an experiment.

These computations are very easily made, with the assistance of a table of logarithms, in the following manner.

Supposing CD, CG, and GH, to have been determined by actual observation; and that it were required to ascertain, by computation, the absciss CE, corresponding to any given intermediate ordinate EF, or, (which is the same thing,) to determine at *what time* the cooling body was at any given intermediate temperature ($= EF$) between that ($= CD$) which it was found by observation to have at the point C, and that ($= GH$) which it was found to have after the time represented by the line CG had elapsed;

It is $\log. CD - \log. GH$, is to CG as 1 to m ; ($=$ modulus $=$ the subtangent of the curve at the point D.)* And $CE = m \times \log. CD - \log. EF$.

* The subtangent shows in what time the instrument would cool down to the temperature of the air in which it is placed, were its velocity of cooling at the point D to be continued *uniformly* from that point; and, as the subtangent of the logarithmic curve is *constant*, if PQ were the logarithmic curve, it would follow, that the velocity

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If, for instance, in the experiment of the 11th March, (the details of which have just been given,) the time when the instrument No. 2, in cooling, passed the important point of 94° , had not been observed, this neglect might have been supplied, by computation, in the following manner.

It is $CD = 94\frac{3}{4}^{\circ}$, the nearest *observed* temperature higher than $EF (= 94^{\circ})$ and $GH = 90\frac{1}{4}^{\circ}$, the nearest observed temperature below that of 94° ; and $CG = 15$ minutes, or 900 seconds, = the time elapsed between the two observations.

$$\text{It is } \log. 94\frac{3}{4} = 1.9765792$$

$$\text{And } \log. 90\frac{1}{4} = 1.9554472$$

$$\text{Log. } CD - \log. GH = 0.0211320$$

$$\text{And } 0.0211320 \text{ is to } 900 (= CG) \text{ as } 1 \text{ to } 42590 = m.$$

$$\text{And again, } \log. 94\frac{3}{4} = 1.9765792$$

$$\text{Log. } 94 = 1.9731279$$

$$\text{Log. } CD - \log. EF = 0.0034513$$

$42590 \times 0.0034513 (= m \times \log. CD - \log. EF) = 147$ seconds, = 2 minutes and 27 seconds; which differs very little from $2\frac{1}{2}$ minutes, the observed time.

If, from the temperature observed at $11^h 30 \text{ min.} = 86\frac{1}{4}^{\circ}$, and the temperature observed at $11^h 45 \text{ min.} = 82\frac{1}{2}^{\circ}$, and the time which elapsed between these two observations, (= 15 minutes) we were to determine, by computation, the time when the instrument was at the temperature of 84° , (the lower point of the standard interval of 10 degrees answering to the temperature of

with which a hot body cools in a fluid medium, is every where such, that were *that velocity* to be continued uniformly, the body would be cooled down to the temperature of the medium, *in the same time*, whatever might be the excess of the temperature of the hot body above that of the medium, at the moment when its velocity of cooling became uniform.

the air, $=44^{\circ}$, in which the instrument was cooled,) it will turn out, 8 minutes and 55 seconds after $11^h 30$ min. The observed time was $11^h 39$ minutes; which differs from the computed time no more than 5 seconds.

If it were strictly true, as a very great philosopher and mathematician has advanced, that the velocity with which a hot body exposed to cool in a cold fluid medium parts with its heat, is as the difference of the temperatures of the body and of the medium, it is most certain, that the curve PQ could be no other than the logarithmic curve. Perhaps it may be so in fact, and that the variations from it which my experiments indicated, were owing solely to the imperfection of the divisions of our thermometers. If it be so, it is not impossible to divide the scale of a thermometer in such a manner as to indicate with certainty *equal increments of heat*, as thermometers ought to do; but this is not the proper place to enlarge on this subject. I may perhaps return to it hereafter.

Passing over in silence, a number of experiments I made in order to get thoroughly acquainted with my new instruments, and to assure myself that the results of similar experiments made with them were uniform, and might be depended on, I shall now proceed to give an account of several experiments made with pointed views, the results of some of which were very interesting.

Experiment No. 1. The large cylindrical vessel No. 1, with its ends covered with warm clothing, in the manner before described, and its vertical sides (which were polished, and very clean and bright) exposed naked to the air, was filled with water nearly boiling hot, and placed on its wooden stand, on a table, in a

large quiet room, to cool; the air of the room being at the temperature of 45° FAHRENHEIT.

Another cylindrical vessel, No. 2, in all respects like No. 1, and with its ends covered in the same manner, but with its vertical sides covered with a single covering of fine Irish linen, (such as is sold in London for about *4s. per yard*,) closely applied to the body of the instrument, was filled with hot water at the same time, and placed on the same table to cool.

This experiment lasted many hours; and, in that period, the temperature of the water, in each of the instruments, was carefully observed, and noted down, a great number of times.

The result of this experiment (the details of which have already been given) was very remarkable.

While the instrument No. 1, whose sides were *naked*, employed 55 minutes in cooling from the point of 94° to that of 84° , the instrument No. 2, whose sides were *covered with linen*, cooled through the same interval in $36\frac{1}{2}$ minutes.

Hence it appears, that clothing may, in some cases, expedite the passage of heat out of a hot body, instead of confining it in it.

Desirous of seeing whether the same covering would, or would not, expedite the passage of heat *into* the instrument; after having suffered both instruments to cool down to the temperature of about 42° , I removed them into a warm room, in which the air was at the temperature of 62° ; and I found that the instrument No. 2, which was clothed, acquired heat considerably faster than the other, No. 1, which was naked.*

* The details of this experiment (which was made on the 11th of March, 1803) may be seen in page 83.

The discovery of these extraordinary facts surprised me, and excited all my curiosity; and I immediately set about investigating their cause.

As it is well known that air adheres with considerable obstinacy to the surfaces of some solid bodies, I conceived it to be possible, that the particles of air in immediate contact with the surface of the cylindrical vessel No. 1, might in fact be so attached to the metal as to adhere to it with some considerable force; and, if that were the case, as confined air is known to constitute a very warm covering, it appeared to me to be possible, that the cooling of the vessel No. 1, might have been retarded by such an invisible covering of confined air; which covering, in the experiment with the vessel No. 2, had been displaced, and in a great measure driven away, by the colder covering, of linen, by which the body of the instrument was closely embraced.

I conceived that the linen must have accelerated the cooling of the instrument, either by facilitating the approach of a succession of fresh particles of cold air, or by increasing the effects of *radiation*; and, with a view to elucidate that important point, the following experiments were made.

Exper. No. 2. Removing the linen with which the instrument No. 2 was clothed, I now covered the sides of that instrument with a thin transparent coating of glue; and, when it was quite dry and hard, I again filled the two instruments (No. 1 and No. 2) with hot water, and observed the times of their cooling as before.

Result, or time of cooling 10 degrees, reckoned from the 50th to the 40th degree above the temperature of the air in which the instruments were exposed to cool.

Instrument No. 1, sides *naked* - - - 55 min.

Instrument No. 2, sides *covered with one coating of glue* 43 $\frac{1}{4}$ min.

When we consider this experiment with attention, we shall find reason to conclude, that if it were by facilitating the approach and temporary contact of a succession of fresh particles of the cold air of the room to the surface of the glue, (which was now in fact become the surface of the hot body,) that the cooling of the instrument was accelerated, the metal being as completely covered, and the air, supposed to be attached and fixed to its surface, as completely excluded by one coating of the glue as it could be by two, or more, two coatings could not possibly accelerate the cooling of the instrument more than one; but if, on the other hand, the cooling of the instrument in this experiment was accelerated, not by facilitating and accelerating the motions of the circumambient cold air, but by facilitating and increasing those *radiations* which are known to proceed from hot bodies, I conceived that two coatings of the glue might possibly accelerate the cooling of the vessel more than one. In order to put this conjecture to the test, I made the following decisive experiment.

Exper. No. 3. I now gave the instrument No. 2 a second coating of glue; and, when it was thoroughly dry, I repeated the experiment last mentioned, with the above variation; when I found the results to be as follows.

		Time of cooling the 10 degrees in question.
The instrument No. 1, <i>naked</i> metal	- -	55 $\frac{1}{3}$ min.
No. 2, <i>covered</i> with two coatings of glue		37 $\frac{5}{6}$ min.

Finding that two transparent coatings of glue facilitated the cooling of this instrument even more than one coating, I washed off all the glue with warm water; then, making the instrument as clean and bright as possible, I covered its sides with

a coating of very fine, transparent, and colourless spirit varnish; and, after this coating of varnish had become quite dry and hard, I repeated the experiment above-mentioned; and, finding that this covering, like that of glue, expedited the cooling of the instrument, I first added a *second* coating of the varnish, and repeated the experiment again, and then added two coatings more, making *four* in all. Finding that the cooling of the instrument was more and more rapid, as the thickness of the varnish was increased, I now added four coatings more, making *eight* coatings in the whole, giving time for each new coating to dry thoroughly, before the next was applied; but I found, on repeating the experiment with this thick covering of varnish, that I had passed the limit of thickness which produced the greatest effect.

In order that the results of these experiments, with coatings of different thicknesses of spirit varnish, may be seen at one view, I shall here place them all together; and I shall place by the side of each, the result of the standard experiment, which was made at the same time, with the instrument No. 1, the sides of which were *naked*.

		Time employed in cooling through the given interval of 10 degrees.	
		Instrument No. 1, <i>varnished.</i>	Instrument No. 2, <i>naked.</i>
<i>Exper. No. 4.</i>	1 coating of varnish -	42 min.	55½ min.
<i>Exper. No. 5.</i>	2 coatings - - -	35¾	55¼
<i>Exper. No. 6.</i>	4 coatings - - -	30¼	55½
<i>Exper. No. 7.</i>	8 coatings - - -	34¼	55

Exper. No. 8. Desirous of finding out what effect *colour* would produce, I now painted the sides of the instrument No. 2 *black*, with lamp-black mixed up with size, (this paint being laid

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upon the eighth coating of the varnish,) and, repeating the experiment, its results were as follows.

	Time employed in cooling through the given interval.
The instrument No. 1, <i>naked</i> - - -	55 $\frac{1}{4}$ min.
Instrument No. 2, covered with 8 coatings of } varnish, and painted <i>black</i> }	34 min.

Exper. No. 9. Finding that the painting of this thick coating of varnish *black*, rendered the covering still colder, or accelerated the cooling of the instrument, I now washed off the black paint, with warm water; then, washing off all the varnish with hot spirit of wine, I painted the metallic sides of the instrument of a black colour, with lamp-black and size; and, when the paint was quite dry, I repeated the experiment so often mentioned; when the results were as follows.

	Time employed in cooling through the given interval.
The instrument No. 1, sides <i>naked</i> - - -	55 $\frac{1}{6}$ min.
No. 2, <i>painted black</i> - - -	35 min.

Exper. No. 10. In order to find out whether the *black* colour had any particular efficacy in expediting the cooling of the instrument, or whether another colouring substance would not produce the same effect, when mixed up with the same size, I now washed off the black paint, and painted the sides of the instrument *white*, with whiting mixed up with size; and, on repeating the experiment, the results were as follows.

	Time of cooling through the given interval.
The instrument No. 1, <i>naked</i> - - -	55 $\frac{1}{3}$ min.
No. 2, <i>painted white</i> - - -	36 min.

As, in both the two last experiments, it was found necessary

to paint the body of the instrument three or four times over, in order to cover the polished metal so completely as to prevent its shining through the paint; this of course occasioned the surface of the metal to be covered with a thick coating of size, which, no doubt, affected very sensibly the results of the experiment, and rendered it impossible to determine, in a satisfactory manner, what the effects really were, which were produced by the *different colours* used in the two experiments.

Exper. No. 11. With a view to throw some more light on this interesting subject, having washed off the paint from the instrument No. 2, I now rendered its sides of a perfectly deep black colour, by holding it over the flame of a wax candle; and, repeating the usual experiment, the results were as follows.

				Time of cooling through the standard interval.
The instrument No. 1, <i>naked</i>	-	-	-	55 $\frac{7}{8}$ min.
No. 2, <i>blackened</i>	-	-	-	36 $\frac{1}{8}$ min.

In order to ascertain the quantity of matter which composed this black covering, I weighed a small piece of clean and very fine linen; and, having wiped off with it all the black matter from the body of the instrument No. 2, in such a manner that the whole of it remained attached to the linen, I weighed it again, and by that means discovered that the whole of this black substance, which had so completely covered the sides of the instrument (a surface of polished brass = 50 superficial inches) that the metal did not shine through it in any part, weighed no more than $\frac{1}{18}$ of a grain Troy.

How this very thin covering, which, if the specific gravity of the black matter were only equal to that of water, would amount to no more than $\frac{1}{4509}$ of an inch in thickness, could expedite

the cooling of the instrument, in the manner it was found to do, is what still remains to be shown : but, before I proceed any farther in these abstruse enquiries, I shall make a few observations relative to the results of the foregoing experiments.

Although we may with safety presume, that the velocities with which the heat escaped *through the sides of the instruments*,* were nearly as the times inversely taken up in cooling through the given interval of 10 degrees ; yet, as some heat must have made its way, in the course of the experiment, *through the ends of the instrument*, notwithstanding all the care that was taken to prevent it, by covering them up with warm clothing, it is necessary, in order to be able to compare the results of the preceding experiments in a satisfactory manner, to find out how much of the heat made its escape through the covered ends of the instruments, during the time the instruments were cooling through the interval in question.

In order to determine that point, I now removed the covering from the ends of the instrument No. 1 ; and, when it was quite naked, I found, on making the experiment, that it cooled through the given interval in $45\frac{1}{2}$ minutes.

When its two ends and its cylindrical neck were covered up

* I have found myself obliged in this, as in many other places, to make use of language which is far from being as correct as I could wish. I do not believe that heat ever *makes its escape* in the manner here indicated ; but I could not venture to use uncommon expressions, in pointing out the phenomena in question, however well adapted such expressions might be to describe the events which really take place. If it should be found that *caloric*, like *phlogiston*, is merely a creature of the imagination, and has no real existence, (which has ever appeared to me to be extremely probable,) in that case, it must be incorrect to speak of heat as *making its escape* out of one body, and *passing* into another : but how often are we obliged to use incorrect and figurative language, in speaking of natural phenomena !

with warm clothing, I found, by taking the mean of the results of several experiments, that it required $55\frac{1}{2}$ minutes to cool through the same interval.

On measuring the instrument with care, I found its dimensions to be as follows.

	Inches.
Diameter of the body of the instrument -	$= 4.03$
Length of the body - - - -	$= 3.96$
Diameter of the neck of the instrument -	$= 0.8$
Length of the neck - - - -	$= 4.$

The superficies of the different parts of the instrument are therefore as follows.

Superficies of the vertical sides of the body ($= 4.03 \times 3.14159 \times 3.96$) $= 50.136$ inches.

Superficies of the flat circular bottom of the instrument, ($= 4.03 \times 3.14159 \times \frac{4.03}{4}$) $= 12.755$ inches; deducting nothing for that part which is covered by the end of the tube, which serves as a support for the instrument.

Superficies of the flat circular top of the instrument, (after deducting 0.502 of a superficial inch, for the circular hole in its centre, made to receive the lower end of the cylindrical neck,) $= 12.253$ inches.

Superficies of the cylindrical neck of the instrument ($= 0.8 \times 3.14159 \times 4$) $= 10.051$ inches.

Supposing now, that the heat passes with equal velocity through the surface of all the different parts of the instrument, when the instrument is naked, we can determine the quantity of heat which escaped through the ends and neck of the instrument, in the experiments in which those parts of the instrument were covered with warm clothing.

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The whole of the metallic surface exposed to the air, in the experiments made with the instrument when it was quite naked, amounted to 85.195 superficial inches; namely;

Surface of its vertical sides	-	-	= 50.136 inches.
of its lower end	-	-	= 12.755
of its upper end	-	-	= 12.253
of its neck	-	-	= 10.051
<hr/>			
Total surface = 85.195 inches.			

When the instrument was exposed quite naked to the air, it was found to cool through the standard interval of 10 degrees, in $45\frac{1}{2}$ minutes.

Assuming now any given number, as the measure of the whole quantity of heat given off by the instrument during the period above-mentioned, we can ascertain what part or proportion of that quantity passed off through the sides of the instrument; and what part of it must have made its escape through its ends, and through the sides of its neck.

As the quantities of heat given off are supposed to have been as the quantities of surface exposed to the air, if we suppose the whole quantity of heat lost by the instrument to be = 10000 parts, the quantity which passed through the vertical sides of the instrument in $45\frac{1}{2}$ minutes, in the experiment, must have amounted to 5885 parts. For, the whole of the surface of the instrument, = 85.195 superficial inches, is to the whole of the heat given off, = 10000, as the surface of the vertical sides of the instrument, = 50.136 superficial inches, to the quantity of heat which must have passed off through that surface in the given time, = 5885.

Now, as we may with safety conclude, that the quantity of

heat which passes off through a *given surface* must be as the times elapsed, all other circumstances being the same, we can determine how much of the heat given off by the instrument, in those experiments in which its ends were covered, passed through the sides of the instrument; and, consequently, how much of it must have made its way through its ends and neck, notwithstanding their being covered.

The instrument with its ends and neck covered up with eider-down, furs, &c. was found to cool through the standard interval of 10 degrees in $55\frac{1}{2}$ minutes. Now, as only 5885 parts of heat were found to pass through the naked vertical sides of the instrument in $45\frac{1}{2}$ minutes, no more than 7015 parts could have passed through the same surface in $55\frac{1}{2}$ minutes; consequently, the remainder of the heat lost by the instrument, in the experiment in question, amounting to 2985 parts, must necessarily have made its way through the covered ends and neck of the instrument, in the given period, $55\frac{1}{2}$ minutes.

Taking it for granted that these computations are well founded, we may now proceed to a more exact determination of the relative quantities of heat which made their way through the sides of the instrument No. 2, when its sides were exposed naked to the air, and when they were covered with the different substances which appeared to facilitate the escape of the heat.

In the experiment No. 11, when the sides of the instrument were made quite black, by holding it over the flame of a wax candle, the instrument cooled through the standard interval of 10 degrees in $36\frac{1}{8}$ minutes.

In that time, a quantity of heat = 1942 parts, must have passed off through the covered ends and neck of the instrument; for, if a quantity = 2985 parts could pass off that way in $55\frac{1}{2}$

minutes, the quantity above-mentioned ($= 1942$ parts) must have escaped in $36\frac{1}{8}$ minutes.

This quantity, $= 1942$ parts, taken from the whole quantity, $= 10000$ parts, lost by the instrument in cooling through the interval in question, leaves 8058 parts, for the quantity which made its escape through the sides of the instrument, in the experiment in question.

Now, if a quantity of heat $= 7015$ parts, requires $55\frac{1}{2}$ minutes to make its way through the naked sides of the instrument, (as we have just seen,) it would require $63\frac{3}{4}$ minutes, for the quantity in question, $= 8058$ parts, to pass off through the same surface.

But, when that surface was blackened over the flame of a candle, that quantity of heat passed off through it in $36\frac{1}{8}$ minutes.

Hence it appears, that the velocity with which heat is given off from the naked surface of a heated metal exposed to cool in the air, is to the velocity with which it is given off by the same metal when its surface is blackened in the manner above described, as $36\frac{1}{8}$ to $63\frac{3}{4}$, or as 5654 to 10000 , very nearly; for the velocities are as the times of cooling, inversely.

Again, in the experiment No. 6, the sides of the instrument No. 2 being covered with four coatings of spirit varnish, the instrument was found to cool through the given interval of 10 degrees in $30\frac{1}{4}$ minutes,

In that time, a quantity of heat $= 1627$ parts, must have made its way through the covered ends of the instrument; and the remainder, $= 8373$ parts, must have made its way through its varnished sides.

This quantity, $= 8373$ parts, would have required $66\frac{1}{4}$ minutes, to have made its way through the naked sides of the instrument; and, as it actually made its way through the varnished sides of

the instrument in $30\frac{1}{4}$ minutes, it appears that the velocity with which the heat was given off from the naked metallic surface, was to the velocity with which it was given off from the same surface covered with four coatings of spirit varnish, as $66\frac{1}{4}$ to $30\frac{1}{4}$, or as 10000 to 4566.

Without pursuing these computations any farther, at present, and without stopping to make any remarks on the curious facts they present to us, I shall hasten to experiments from the results of which we shall obtain more satisfactory information. But, before I proceed any farther, I must give an account of an instrument I contrived for measuring, or rather for *discovering*, those very small changes of temperature in bodies, which are occasioned by the radiations of other neighbouring bodies, which happen to be at a higher, or at a lower temperature.

This instrument, which I shall take the liberty to call a *thermoscope*, is very simple in its construction. Like the hygrometer of Mr. LESLIE, (as he has chosen to call his instrument,) it is composed of two glass balls, attached to the two ends of a bent glass tube; but the balls, instead of being near together, are placed at a considerable distance from each other; and the tube which connects them, instead of being bent in its middle, and its two extremities turned upwards, is quite straight in the middle, and its two extremities, to which its two balls are attached, are turned perpendicularly upwards, so as to form each a right angle with the middle part of the tube, which remains in a horizontal position.

At one of the elbows of this tube, there is inserted a short tube, of nearly the same diameter, by means of which, a very small quantity of spirit of wine, tinged of a red colour, is introduced into the instrument; and, after this is done, the end of

this short tube (which is only about an inch long) is sealed hermetically; and all communication is cut off, between the air in the balls of the instrument and in its tube, and the external air of the atmosphere.

A small *bubble* of the spirit of wine (if I may be allowed to use that expression) is now made to pass out of the short tube, into the long connecting tube; and the operation is so managed, that this bubble (which is about $\frac{3}{4}$ of an inch in length) remains stationary, at or near the middle of the horizontal part of the tube, *when the temperature (and consequently the elasticity) of the air in the two balls, at the two extremities of the tube, is precisely the same.*

By means of a scale of equal parts, attached to the horizontal part of the connecting tube, the position of the bubble can be ascertained, and its movements observed.

If now, the bubble being at rest in its proper place, one of the balls of the instrument be exposed to the calorific rays which proceed in all directions from a hot body, while the other ball is defended from those rays by a screen, the air in the ball so exposed to the action of these rays, will be heated; and, its elasticity being increased by this additional heat, its pressure will no longer be counterbalanced by the elasticity of the colder air in the other ball, and the bubble will be forced to move out of its place, and to take its station nearer to the colder ball.

By presenting two hot bodies, at the same time, to the two balls of the instrument, taking care that each ball shall be defended from the action of the hot body presented to the opposite ball, the distances of these hot bodies from their respective balls may be so regulated, that their actions on those balls may be equal, however the temperatures of those hot bodies may differ,

or however different may be the quantities, or intensities, of the calorific rays which they emit.

The instrument will show, with the greatest certainty, when the actions of these hot bodies on their respective balls are equal; for, until they become *unequal*, the bubble will remain immoveable in its place.

And, when the actions of two hot bodies on the instrument are equal, the relative intensities of the rays they emit may be ascertained, by the distances of the bodies from the balls of the instrument.

If their distances from their respective balls are equal, the intensities of the rays they emit must of course be equal.

If those distances are unequal, the intensities will probably be as the squares of the distances, inversely.

A distinct and satisfactory idea may be formed, of the instrument I have been describing, from Figure 2.

AB is a board, 27 inches long, 9 inches wide, and 1 inch thick, which serves as a support for the bent tube CDE, at the two extremities of which the two balls are fixed. The two projecting ends of the tube, C and E, which are in a vertical position, are each 10 inches long; and the horizontal part D of the tube, which is fastened down on the board, is 17 inches in length.

The balls are each 1.625 inches in diameter. The diameter of the tube is such, that 1 inch of it in length would contain 15 grains Troy of mercury.

The pillar F, which, by means of a horizontal arm projecting from it, serves for supporting the circular vertical screen represented in the figure, is firmly fixed in the board AB.

This circular screen (which is made of pasteboard, covered

on both sides with gilt paper) serves for preventing one of the balls of the instrument from being affected by the calorific rays proceeding from a hot body which is presented to the opposite ball.

Besides the circular screen represented in the figure, several other screens are used in making experiments; for the instrument is so extremely sensible, that the naked hand presented to one of the balls, at the distance of several inches, puts the bubble in motion; and it is affected very sensibly by the rays which proceed from the person who approaches it to make the experiments, unless care be taken, by the interposition of screens, to prevent those rays from falling on the balls. These screens can be best and most readily made, by providing light wooden frames, about 2 feet square, and half an inch in thickness, and covering them on both sides, first with thick cartridge paper, and then with what is called gilt paper; the metallic substance (copper) with which one side of the paper is covered being on the outside.

To support a moveable screen of this kind in a vertical position, it must of course be provided with a foot or stand. Those I use, are fastened to one side of a pillar of wood, by two screws; one of which passes through the centre of the screen, where the cross bars belonging to the frame of the screen meet; and the other through the middle of the piece of wood which forms the bottom of the screen. This pillar of wood, which is turned in a lathe, is $12\frac{1}{2}$ inches high, and is firmly fixed, at its lower end, in a piece of wood, 8 inches square, and 1 inch thick, which serves as a stand or foot, for supporting it.

As, in making experiments with this *thermoscope*, it is frequently necessary to remove the hot bodies, that are presented

to it, farther from it, or to bring them nearer to it ; in order that this may be done easily, and expeditiously, by one person, and without its being necessary for him to remove his eye from the bubble, (which he should constantly have in his view,) I make use of a simple machine, which I have found to be very useful.

It is a long and shallow wooden box, open at both ends. It is 6 feet long, 12 inches wide, and 5 inches deep, measured on the outside: its vertical sides are made of $1\frac{1}{2}$ -inch deal; its bottom and top, of inch deal. A part only of the top or cover of this box is fixed down on the sides, and is immoveable. The part of the cover which is fixed, and on which the thermoscope is placed, occupies the middle of the box, and is 13 inches in length. On the right and left of this fixed part, the top of the box is covered by a sliding board, 2 feet 3 inches long, which passes in deep grooves, made to receive it, in the sides of the box. A rack is fixed to the under side of each of these sliding boards; and there is a small cog wheel in the box, the axis of which passes through the sides of the box, and is furnished with a winch in the front of the box. By turning round these wheels, by means of their winches, (both of which can be managed by the same person, at the same time,) the sliders may be moved backwards and forwards, at pleasure.

In order to ascertain with facility and dispatch, the distances of the hot bodies from their respective balls, the top of the front side of the wooden box is divided into inches, on each side of the fixed part of the cover of the box; and there is a *nonius* belonging to each of the sliders, which is placed in such a manner as to indicate, at all times, the exact distance of the hot body from its corresponding ball.

The level of the upper surface of that part of the cover

which is fixed, is about $\frac{1}{8}$ of an inch higher than the level of the upper surface of the sliders; in order that, when a thermoscope longer than this fixed part is placed on it, the sliders may pass freely under its two projecting ends, without deranging it.

It is evident, from this description, that, by placing the thermoscope on the fixed part of the cover of the box, with its two balls in a line parallel to the axis of the box, and by placing the two hot bodies presented to the two balls of the instrument (elevated to a proper height) on stands set down on the sliders, an observer, by taking the two winches in his hands, keeping his eye fixed on the bubble, may, with the greatest facility, so regulate the distances of the hot bodies from their respective balls, that the bubble shall remain immoveable in its place.

In order to be able to ascertain precisely the temperatures of the hot bodies presented to this instrument, and in order that their surfaces might be equal, two equal cylindrical vessels, of thin sheet brass, with oblique cylindrical necks, were provided, of the form represented in Figure 3.

This cylindrical vessel, which is placed in a horizontal position, in order that its flat bottom may be presented, *in a vertical position*, to one of the balls of the thermoscope, is so fixed to a wooden stand, of a peculiar construction, that it may be raised, or lowered, at pleasure. This is necessary, in order that its axis may be in the continuation of a line passing through the centres of the two balls of the thermoscope.

This cylindrical vessel is 3 inches in diameter, and 4 inches in length; and its oblique cylindrical neck is 0.86 of an inch in diameter, and 3.8 inches in length.

The neck of this vessel is inserted *obliquely* into its cylindrical

body, in order that the water with which it is occasionally filled may not run out of it, when the body of the vessel is laid down in a horizontal position, in the manner represented in the above-mentioned figure.

A thermometer, with a cylindrical bulb 4 inches in length, being inserted into the body of this vessel, through its neck, shows the temperature of the contained water.

Care is necessary, in constructing a thermoscope, to choose a tube of a proper diameter: if its bore be too small, it will be found very difficult to keep the spirit of wine in one mass; and, if it be too large, the little horizontal column it forms, (which I have called a bubble,) will be ill defined at its two ends, which will render it difficult to ascertain its precise situation. After a number of trials, I have found, that a tube, the bore of which is of such a size that 1 inch of it in length contains about 15 or 18 grains Troy of mercury, answers best. For a tube of that size, the balls may be about $1\frac{1}{2}$ inch in diameter; and they should both be painted black, with Indian ink, which renders the instrument more sensible.

I have an instrument of this kind, the tube of which is quite filled with spirit of wine, excepting only the space occupied by a small bubble of air, which is introduced into the middle of the horizontal part of the tube; but it does not answer so well as those which contain only a very small quantity of that liquid, sufficient to form a small bubble.

But, without enlarging any farther, at present, on the construction of these instruments, I now proceed to give an account of the experiments for which they were contrived.

Having found abundant reason to conclude, from the results of the experiments of which an account has already been given,

that all the heat which a hot body loses, when it is exposed in the air to cool, is not given off to the air which comes into contact with it; but that a large proportion of it escapes in rays, which do not heat the transparent air through which they pass, but, like light, generate heat only when, and where, they are stopped and absorbed; I suspected that, in every case when, in the foregoing experiments, the cooling of my instruments was expedited by coverings applied to their metallic surfaces, those coverings must, by some means or other, have facilitated and accelerated the emission of calorific rays from the hot surface.

Those suspicions implied, it is true, the supposition that different substances, heated to the same temperature, emit unequal quantities of calorific rays; but I saw no reason why this might not be the case in fact; and I hastened to make the following experiments, which put the matter beyond all doubt.

Exper. No. 12. Two equal cylindrical vessels, made of sheet brass, and polished very bright, each 3 inches in diameter, and 4 inches long, suspended by their oblique necks, in a horizontal position, (being placed on their wooden stands,) were filled with water at the temperature of 180° ; and their circular flat bottoms were presented, in a vertical position, to the two balls of the thermoscope, at the distance of 2 inches.

When the two hot bodies were presented, at the same moment, to the two balls of the instrument, or, what was still better, when two screens were placed before the two balls, at the distance of about an inch, and, after the hot bodies were placed, these screens were both removed at the same instant, the small column of spirit of wine, which I have called a *bubble*, remained immoveable in its place, in the middle of the horizontal part of the tube of the instrument.

If one of the hot bodies was now brought nearer the ball to which it was presented, (the other hot body remaining in its place,) the bubble immediately began to move from the hot body which was advanced forward, towards the opposite ball, to which the other hot body was presented.

If, instead of advancing one of the hot bodies nearer the ball to which it was presented, it was drawn backward to a greater distance from it, the action of its calorific rays on the ball was diminished by this increase of distance; and, being overcome by the action of the rays from the hot body presented to the opposite ball, (at a smaller distance,) the bubble was forced out of its place, and obliged to move towards the ball which had been drawn backward.

When one of the hot bodies only was presented to one of the balls, the bubble was immediately put in motion; and, by bringing the hot body nearer to the ball, it might be driven quite out of the tube, into the opposite ball; this, however, should never be done, because it totally deranges the instrument, as it is easy to perceive it must do.

Having, by these trials, ascertained the sensibility and the accuracy of my instrument, I now proceeded to make the following decisive experiment.

Exper. No. 13. Having blackened the flat circular bottom of one of the cylindrical vessels, by holding it over the flame of a wax candle, I now filled both vessels again with water at the temperature of 180° F. and presented them, as before, to the two opposite balls of the instrument, at equal distances.

The bubble was instantly driven out of its place, by the superior action of the blackened surface; and did not return to its former station, till after the vessel which was blackened

had been removed to more than 8 inches from the ball to which it was presented; the other vessel, which had not been blackened; remaining in its former situation, at the distance of 2 inches from its ball.

The result of this experiment appeared to me to throw a new light on the subject which had so long engaged my attention; and to present a wide and very interesting field for farther investigation.

I could now account, in a manner somewhat more satisfactory, for those appearances in the foregoing experiments which were so difficult to explain,—for the acceleration of the passage of the heat out of my instruments, which resulted from covering them with linen, varnish, &c. and I immediately set about making a variety of new experiments, from which I conceived I should acquire a farther insight into those invisible mechanical operations which take place when bodies are heated and cooled.

Finding so great a difference in the quantities of calorific rays which are thrown off by the polished surface of a metal, when exposed *naked* to the cold air, and when *blackened*, I now proceeded to make experiments, to ascertain whether or not all those substances with which the sides of my cylindrical vessels had been covered, and which had been found to expedite the cooling of those instruments, would also facilitate the emission of calorific rays from the surfaces of the instruments I presented to the balls of my thermoscope; and I found this to be the case in fact.

As the results of all these experiments proved, in the most decisive manner, that all the substances which, when applied to the metallic surfaces of my large cylindrical vessels, had expedited their cooling, facilitated and expedited the emission of

calorific rays, I could no longer entertain any doubts respecting the agency of *radiation*, in the heating and cooling of bodies. Many important points however still remained to be investigated, before distinct and satisfactory ideas could be formed, respecting the nature of those rays, and the mode of their action.

I had hitherto made use of but one metal (brass) in my experiments; and that was not a simple, but a compound metal. The first subject of enquiry which presented itself, in the prosecution of these researches, was to find out whether or not similar experiments made with other metals would give similar results.

Exper. No. 14. Procuring from a gold-beater a quantity of leaf gold and leaf silver, about three times as thick as that which is commonly used by gilders, I covered the surfaces of the two large cylindrical vessels, No. 1 and No. 2, with a single coating of oil varnish; and, when it was sufficiently dry for my purpose, I gilt the instrument No. 1 with the gold leaf, and covered the other, No. 2, with silver leaf. When the varnish was perfectly dry and hard, I wiped the instruments with cotton, to remove the superfluous particles of the gold and silver, and then repeated the experiment so often mentioned, of filling the instruments with boiling hot water, and exposing them to cool, in the air of a large quiet room.

The time of cooling through the given interval of 10 degrees, was just the same as it was before, when the natural surface of these brass vessels was exposed *naked* to the air. I repeated the experiment several times, but could not find that the difference in the metals made any difference in the times of cooling.

Exper. No. 15. Not satisfied to rest the determination of so important a point on a trial with three metals only, brass, gold,

and silver, I now provided myself with two new instruments, the one made of lead, and the other covered with tinned sheet iron, improperly, in England, called tin.

As the *conducting power* of lead, with respect to heat, is much greater than that of any other metal, I conceived that, if the *radiation* of a body were any way connected with its *conducting power*, the cooling of the water contained in the leaden vessel, would necessarily be either more or less rapid than in a vessel constructed of any other metal.

The result of this experiment, as also the results of several others similar to it, showed that heat is given off with the same facility, or with the same celerity, from the surfaces of all the metals.

Is not this owing to their being all equally wanting in *transparency*? And does not this afford us a strong presumption that heat is, in all cases, excited and communicated by means of radiations, or *undulations*, as I should rather choose to call them?

I am sensible, however, that there is another and most important question to be decided, before these points can be determined; and that is, whether bodies are cooled in consequence of the rays they emit, or by those they receive?

The celebrated experiment of Professor PICTET, which has often been repeated, appears to me to have put the fact beyond all doubt, that rays, or emanations, which, like light, may be concentrated by concave mirrors, proceed from cold bodies; and that these rays, when so concentrated, are capable of affecting, in a manner perfectly sensible, a delicate air thermometer.

One of the objects I had principally in view, in contriving the

before described instrument, which I have called a thermoscope, was to investigate the nature and properties of those emanations; and to find out, if possible, whether they are not of the same nature as those calorific rays which have long been known to proceed from hot bodies.

My first attempts, in these investigations, were to ascertain the existence of those emanations universally; and to discover what visible effects they might be made to produce, independently of concentration by means of concave mirrors.

Exper. No. 16. My two horizontal cylindrical vessels, of sheet brass, (of the same form and dimensions,) having been made very clean and bright, were fixed to their stands; and, being elevated to a proper height to be presented to the balls of the thermoscope, were set down near that instrument, (which was placed on a table in a large quiet room,) where they were suffered to remain several hours, in order that the whole of this apparatus might acquire precisely the same temperature.

Day-light was excluded, by closing the window-shutters; and, in order that the thermoscope might not be deranged by the calorific rays proceeding from the person of the observer, on his entering the room to complete the intended experiments, screens were previously placed before the instrument, in such a manner that its balls were completely defended from those rays.

Things having been thus prepared, I entered the room as gently as possible, in order not to put the air of the room in motion, and, approaching the thermoscope, presented first one, and then the other cylindrical vessel, to one of the balls of the instrument; but it was not in the least degree affected by them,

the bubble of spirit of wine remaining immoveably in the same place.

Exper. No. 17. Having assured myself, by these previous trials, that the instrument was not sensibly affected by a bright metallic surface being presented to it, provided the temperature of the metal and that of the instrument were the same, I now withdrew one of the cylindrical vessels, and, taking it into another room, I filled it with pounded ice and water.

Entering the room again, I now presented the flat vertical bottom of this horizontal cylindrical vessel, filled with ice and water, to one of the balls of the thermoscope, at the distance of four inches.

The bubble of spirit of wine began instantly to move, with a slow regular motion, towards the cold body; and, having advanced in the tube about an inch, it remained stationary.

On bringing the cold body nearer the ball to which it was presented, the bubble was again put in motion, and advanced still farther towards the cold body.

Exper. No. 18. Although the result of the foregoing experiment appeared to me to afford the most indisputable proof of the *radiation* of cold bodies, and that the rays which proceed from them have a power of *generating cold* in warmer bodies which are exposed to their influence, yet, in a matter so extremely curious, and of such high importance to the science of heat, I was not willing to rest my enquiries on the result of a single experiment.

In order to vary the substance, or species of matter, presented cold to the instrument, and, at the same time, to remove all suspicion respecting the possibility of the effects observed being

produced by currents of cold air occasioned in the room by the presence of the cold body, I now repeated the experiment with the following variations.

The thermoscope was laid down on one side, so that the two ends of its tube, to which its balls were attached, instead of being vertical, were now in a horizontal position; and the cold body, instead of being presented to the ball of the instrument on one side of it, and on the same horizontal level with it, was now placed *directly under it*, and at the distance of 6 inches.

This cold body, instead of being a metallic substance, was a solid cake of ice, circular, flat, and about 3 inches thick, and 8 inches in diameter. It was placed in a shallow earthen dish, about 9 inches in diameter below, 12 inches in diameter above, at its brim, and 4 inches deep. The cake of ice being laid down on the bottom of the dish, the top of the dish was covered by a circular piece of thick paper, 14 inches in diameter, which had a circular hole in its centre, just 6 inches in diameter.

This earthen dish, containing the ice, and thus covered, was placed perpendicularly under one of the balls of the thermoscope, at such a distance that the centre of the upper surface of the flat cake of ice was 6 inches below the ball.

The result of this experiment was just what might have been expected: the ice was no sooner placed under the ball of the instrument, than the bubble of spirit of wine began to move towards that side where the cold body was placed; and it did not remain stationary, till after it had advanced more than an inch in the tube.

Exper. No. 19. Desirous of discovering whether the surface of a liquid emits frigorific or calorific rays, as solid bodies have been found to do, I now removed the cake of ice from the

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earthen dish, and replaced it with an equal mass of ice-cold water.

The result of this experiment was, to all appearance, just the same as that of the last. The bubble moved towards the cold body, and took its station in the same place where it had remained stationary before. I found reason however to conclude, after meditating on the subject, that although the last experiment proves, in a most decisive manner, that radiations actually proceed from the surface of *water*, yet the proof of the radiation from the surface of ice, afforded by the preceding experiment, is not equally conclusive; for, as the temperature of the air of the room in which these experiments were made, was many degrees above the freezing point, it is possible, and even probable, that the surface of the ice was actually covered with a very thin, and consequently invisible, coating of water, during the whole of the time the experiment lasted.

Finding reason to conclude, that frigorific rays are always emitted by cold bodies, and that these emanations are very analogous to the calorific rays which hot bodies emit, I was impatient to discover, whether all cold bodies, at the same temperature, emit the same quantity of rays, or whether (as I had found to be the case with respect to the calorific rays emitted by hot bodies) some substances emit more of them, and some less.

With a view to the ascertaining of this important point, I made the following experiments.

Exper. No. 20. Having found that a metallic surface, rendered quite black by holding it over the flame of a wax candle, emits a much larger quantity of calorific rays, when hot, than the same metal, at the same temperature, throws off when naked, I was very curious to find out whether blackening the surface of

a cold metal, would or would not increase, in like manner, the quantity of frigorific rays emitted by it.

Having blackened, in the manner already described, the flat bottom, or rather end, of one of my horizontal cylindrical brass vessels with an oblique neck, I filled it with a mixture of ice and common salt; and, filling another vessel of the same kind, the bottom of which was not blackened, with the same cold mixture, I presented them both, at the same instant, and at the same distance, to the two opposite balls of my thermoscope.

The result of this experiment was perfectly conclusive: the bubble of spirit of wine began immediately to move towards the ball to which the *blackened* cold body was presented; indicating thereby, that that ball was more cooled by the frigorific rays which proceeded from the blackened surface, than the opposite ball was cooled by the rays which proceeded from an equal surface of naked metal, at the same temperature.

As this experiment appeared to me to be of great importance, I repeated it several times, and always with the same results; the motion of the bubble, which constituted the index of the instrument, constantly showing that the frigorific rays from the blackened surface were more powerful, in generating cold, than those which proceeded from the naked metal.

The bubble, it is true, did not move so far out of its place as it had done in the experiments in which hot bodies were presented to the balls; but this was not to be expected; for, though I had taken pains, by mixing salt with the ice, to produce as great a degree of cold as I conveniently could, yet still, the difference between the temperature of the balls and that of the bodies presented to them, was much greater when the hot bodies were used, than when the experiments were made with the cold bodies;

and it is evident, that the distance to which the bubble is driven out of its place, must necessarily be greater or less, in proportion as that difference is greater or less.

In those experiments in which the horizontal cylindrical vessels were filled with hot water, and then presented to the balls of the instrument, the temperature of the circular flat surfaces was that of 180° , while the temperature of the air of the room in which those experiments were made, and consequently that of the balls, was about 60° ; the difference amounts to no less than 120 degrees of FAHRENHEIT'S scale; but, in these experiments with cold, the difference of the temperatures at the moment when the cold bodies were first presented to the instrument, did not probably amount to more than 40, or at the most 50 degrees; and, in a very few seconds, it must have been reduced to less than 30 degrees, in consequence of the freezing of the water precipitated by the air of the atmosphere, on the surface of the vessel containing the cold mixture.

This precipitation of water, by the surrounding air, was so copious, that the brilliancy of the polish of the metallic surface was almost instantly obscured by it; and the vessels were very soon covered with a thick coat of ice. These accidents, which were not to be prevented, affected in a very sensible manner the results of the experiment. The bubble, instead of remaining stationary for some time after it had reached the point of its greatest elongation, as it had done in the experiments with hot bodies, had no sooner reached that point, than it began to return back towards the place from which it had set out; and, as often as I wiped off the ice from the surface of the flat end of the vessel which was not blackened, and presented it clean and bright to the ball of the instrument, the bubble began

again to move towards the opposite side; which, by the bye, shows that ice emits a greater quantity of frigorific rays than a bright metallic surface, at the same temperature.

Having frequently observed, on presenting my hand to one of the balls of the thermoscope, that the instrument was greatly affected by the calorific rays which proceeded from it, apparently much more so than it would have been by a much hotter body, of the same quantity of surface, but of a different kind of substance, placed at the same distance, I was extremely curious to find out, whether *animal substances* do not emit calorific (and consequently frigorific) rays much more copiously than other substances; and whether living animal bodies do not emit them in greater abundance than dead animal matter.

The first experiment I made, with a view to the investigation of this particular point, was as simple as its result was striking and conclusive.

Exper. No. 21. Having procured a piece of gold-beater's skin, (which, as is well known, is one of the membranes that line the larger intestines in cattle, and is exceedingly thin,) I moistened it with water; and, applying it, while moist, to the flat circular end of one of my horizontal cylindrical vessels, it remained firmly attached to the surface of the metal, when it became dry. I now filled this vessel, and another, of equal dimensions, the end of which was not covered, with hot water, (at the temperature of 180° ,) and presented them both, at the same moment, to the two balls of the thermoscope, and at the same distance.

The bubble of spirit of wine was immediately driven out of its place, to a great distance; and did not return to its former station, till after the vessel whose end was covered with gold-

beater's skin had been removed to a distance from the ball to which it was presented, which was *five times* greater than the distance at which the other vessel was placed from the opposite ball.

I was induced to conclude, from the result of this interesting experiment, that an animal substance emits *25 times* more calorific rays, than a polished metallic surface of the same dimensions, both substances being at the same temperature.

Exper. No. 22. Having emptied both the vessels used in the last experiment, and refilled them with pounded ice and water, I now presented them again to the thermoscope, at equal distances from their respective balls.

The result of this experiment confirmed the conclusion I had been induced to draw from a former experiment of the same kind, (No. 13,) the motion of the bubble towards the vessel whose surface was covered with gold-beater's skin, showing that the rays which proceeded from that animal substance were considerably more efficacious in producing cold, than those which proceeded from the naked metal.

The radiation of cold bodies appearing to me to have been proved beyond all doubt, by the preceding experiments, I now set about to investigate a very important point, which still remained to be determined: I endeavoured to find out, whether the intensity of the action of the frigorific rays which proceed from cold bodies, or their power of affecting the temperatures of other warmer bodies, *at equal intervals of temperature*, is, or is not, equal to the intensity of the action of the calorific rays which proceed from hot bodies. To ascertain this point, I made the following very simple and decisive experiment.

Exper. No. 23. Having placed the thermoscope on a table, in

the middle of a large quiet room, at the temperature of 72° F. I presented to one of its balls, at the distance of 3 inches, the flat circular end of one of the horizontal cylindrical vessels (A) above described, with an oblique cylindrical neck, this vessel being filled with pounded ice and water; and, at the same moment, an assistant presented to the opposite side of the same ball of the thermoscope, at the same distance, (3 inches,) the flat end of the other similar and equal cylindrical vessel, (B,) filled with warm water at the temperature of 112° F. the opposite ball of the thermoscope being hid and defended, by means of screens, from the actions of the bodies presented to the other ball, as also from the calorific rays which proceeded from the bodies of the persons present.

From this description it appears, that while one of the balls of the thermoscope was so defended by screens that it could not be sensibly affected by the radiations of the neighbouring bodies, the other ball was exposed to the simultaneous action of two equal bodies, at equal distances; (two vertical metallic disks, 3 inches in diameter, placed on opposite sides of the ball, at the distance of 3 inches;) one of these bodies being at the temperature of 32° F. or 40 degrees below that of the ball, while the other was at 112° F. or 40 degrees above the temperature of the ball.

I knew, from the results of former experiments, that this ball would, at the same time, be heated by the calorific rays from the hot body, and cooled by the frigorific rays from the cold body; and I concluded, that if its mean temperature should remain unchanged under the influence of these two opposite actions, that event would be a decisive proof of the equality of the intensities of those actions.

The result of the experiment showed, that the intensities of

those opposite actions were in fact equal; the bubble of spirit of wine, which, by its motion, would have indicated the smallest change of temperature in the ball of the thermoscope, to which the hot and the cold bodies were presented, remained at rest.

On removing the cold body a little farther from the ball, to the distance of $3\frac{1}{2}$ inches, for instance, the hot body remaining in its former station, at the distance of 3 inches, the bubble began immediately to move towards the opposite ball of the thermoscope, indicating an increase of heat in the ball exposed to the actions of the hot and the cold bodies; but, when the hot body was removed to a greater distance, the cold body remaining in its place, the bubble indicated an increase of cold.

The celerity with which the ball of the thermoscope acquired heat, or cold, might be estimated by the velocity with which the bubble of spirit of wine advanced, or retired, in its tube; but, on the most careful and attentive observation, I could not perceive that it moved faster when the ball was acquiring heat, than when it was acquiring cold; provided that the hot and the cold bodies, from which the calorific and frigorific rays proceeded, were at the same relative distances.

From these experiments, which I lately repeated at Geneva, in the presence of Professor PICTET, Mons. de SAUSSURE, M. SENEBIER, and several other persons, we may venture to conclude, that, *at equal intervals of temperature*, the rays which generate cold, are just as real, and just as intense, as those which generate heat; or, that their actions are equally powerful, in changing the temperatures of neighbouring bodies.

On a superficial view of this subject, it might appear extraordinary, that so important a fact as that of the frigorific radiations of cold bodies should have been so long unnoticed, while the

calorific radiations of hot bodies have been so well known ; but, if we consider the matter with attention, our surprise will cease. Those radiations by means of which the temperatures of neighbouring bodies are gradually changed and equalized, are not sensible to our feeling, unless the intervals of temperature be very considerable ; and the constitution of things is such, that while we are often exposed to the influence of bodies heated several thousand degrees (as measured by the thermometer) above the mean temperature of the surface of the skin, it is very seldom that we have opportunities of experiencing the effects of the radiations of bodies much colder than ourselves ; and we have no means of producing degrees of cold which bear any proportion to the intense heats excited by means of fire.

From the result of the experiment of which an account has just been given, it is evident, that we should be just as much affected by the calorific rays emitted by a cannon bullet at the temperature of 160 degrees of FAHRENHEIT'S scale, (=64 degrees above that of the blood,) as by the frigorific rays of an equal bullet, ice cold, placed at the same distance ; and that a bullet at the temperature of freezing mercury, could not affect us much more sensibly, by its frigorific rays, than an equal bullet at the temperature of boiling water would do, by its calorific rays ; but, at these comparatively small intervals of temperature, the radiations of bodies are hardly sensible, and could never have been perceived, much less compared and estimated, without the assistance of instruments much more delicate than our organs of feeling. Hence we see how it happened, that the frigorific radiations of cold bodies remained so long unknown. They were suspected by BACON ; but their existence was first ascertained

by an experiment made at Florence, towards the end of the seventeenth century. And it is not a little curious, that the learned academicians who made that experiment, and who made it with a direct view to determine the fact in question, were so completely blinded by their prejudices respecting the nature of heat, that they did not believe the report of their own eyes; but, regarding the reflection and concentration of cold (which they considered as a negative quality) as *impossible*, they concluded, that the indication of such reflection and concentration, which they observed, must necessarily have arisen from some error committed in making the experiment.

Happily for the progress of science, the matter was again taken up, about twenty years ago, by Professor PICTET; and the interesting fact, which the Florentine academicians would not discover, was put beyond all doubt. But still, this ingenious and enlightened philosopher did not consider the appearances of a reflection of cold, which he observed in his experiments, as being *real*; nor was he led by them to admit the existence of frigorific emanations from cold bodies, analogous to those calorific emanations from hot bodies, which he calls radiant heat. He every where speaks of the reflection of cold (by metallic mirrors) as being merely *apparent*; and it is on that supposition, that the explanation he has given of the phenomena is founded.

On a supposition that the *caloric* of modern chemists has any real existence, and that heat, or an increase of temperature in any body, is caused by an *accumulation* of that substance in such body, the reflection of cold would indeed be impossible; and the supposition that such an event had taken place, would be absurd, and could not be admitted, however striking and convincing the

appearances might be which indicated that event. But, to return from this digression.

Having found that the intensity of the calorific rays emitted by a hot body, at any given temperature, depends much on the surface of such body,—that a polished metallic surface, for instance, throws off much fewer rays than the same surface, at the same temperature, would emit, if painted, or blackened in the smoke of a lamp or candle, I was desirous of finding out, whether the frigorific rays from cold bodies are affected in the same manner, by the same means, and in the same degree.

It was to ascertain that point, that the experiment No. 20 was made; and, although the result of that experiment afforded abundant reason to conclude, that those substances which, when hot, throw off calorific rays in the greatest abundance, actually throw off great quantities of frigorific rays, when they are cold; yet, as the relative quantities of these rays could not be exactly determined by that experiment, in order to ascertain so important a fact, I had recourse to the following simple contrivance.

Exper. No. 24. Having found, by the result of the last experiment, (No. 23,) that the calorific emanations of a circular disk of polished brass, 3 inches in diameter, at the temperature of 112° F. were just counterbalanced by the frigorific emanations of an equal disk of the same polished metal, at the temperature of 32° F. placed opposite to it, so that one of the balls of the thermoscope placed between these two disks, at equal distances, was just as much heated by the one as it was cooled by the other, I now blackened the two disks, by holding them over the flame of a wax candle, and repeated the experiment with them, so blackened.

I knew, from the results of former experiments, that the intensity of the calorific radiations from the hot disk, would be very much increased, in consequence of its surface being blackened; and I was certain, that if the intensity of the frigorific radiations of the cold disk should not be increased in *exactly the same degree*, the ball of the thermoscope, exposed to the simultaneous actions of these two disks, could not possibly remain at the same constant temperature, that of 72° .

The result of the experiment was very decisive: the bubble of spirit of wine remained at rest; which proved, that the intensities of the rays emitted by the two disks, still continued to be equal at the surface of the ball of the thermoscope, which, at equal distances, was exposed to their simultaneous action.

Hence we may conclude, that those circumstances which are favourable to the copious emission of calorific rays from the surfaces of hot bodies, are equally favourable to a copious emission of frigorific rays from similar bodies, when they are cold.

But it is time to consider these emanations in a new point of view. What difference can there be between calorific rays, and frigorific rays? Are not the same rays either calorific, or frigorific, according as the body at whose surface they arrive is hotter, or colder, than that from which they proceed?

Let us suppose three equal bodies, A, B, and C, (the globular bulbs of three mercurial thermometers, for instance,) to be placed, at equal distances, (3 inches,) in the same horizontal line; and let A be at the temperature of freezing water, B at the temperature of 72° F. and C at that of 102° F. The rays emitted by B will be *calorific*, in regard to the colder body A; but, in respect to the hotter body C, they will be frigorific; and,

from the results of the two last experiments, we have abundant reason to conclude, that they will be just as efficacious in heating the former, as in cooling the latter.

Before I proceed to give an account of the experiments which were made with a view to determine the relative quantities of rays emitted from the surfaces of various substances, from living animals, dead animal matter, &c. (which I must reserve for a future communication,) I shall lay before the Society the results of several experiments, of various kinds, which were made with a view to the farther investigation of the radiations of hot and of cold bodies, and of the effects produced by them.

Exper. No. 25. Having found, from the results of the experiments No. 21 and No. 22, that great quantities of rays are thrown off from the surface of the animal substance used in those experiments, (gold-beater's skin,) I now covered the whole of the external surface of one of my large cylindrical passage thermometers (No. 4) with that substance; and, filling it with boiling hot water, exposed it to cool gradually in the air of a large quiet room, in the manner often described in former parts of this Paper; another similar *naked* standard instrument (No. 3) being filled with hot water at the same time, and exposed to cool in the same situation.

The temperature of the air of the room being $51\frac{1}{2}^{\circ}$, the instruments were found to cool through the standard interval of 10 degrees, namely, from $101\frac{1}{2}$ to $91\frac{1}{2}$, in the following times.

No. 4, *covered* with gold-beater's skin, in $27\frac{3}{4}$ minutes.

No. 3, which was *naked*, - - - in 45 minutes.

Exper. No. 26. Being desirous of finding out whether or not the covering of animal matter, which had so remarkably facilitated the cooling of the instrument No. 4, would be equally

efficacious in facilitating the passage of heat *into* the instrument, I suffered both instruments to remain in the cold room all night; and, entering the next morning, at half an hour past seven o'clock, I found the temperature of the water in the *naked* instrument, No. 3, to be $50\frac{1}{8}^{\circ}$: that in the instrument No. 4, which was covered with gold-beater's skin, was $49\frac{1}{4}^{\circ}$; while the air of the room was at 48° .

At 7^h 30^m A. M. I removed both instruments into a warm room, and observed the times of their acquiring heat to be as expressed in the following Table.

Times when the observations were made.	Observed Temperature.		Temperature of the air of the room.
	No. 3, <i>naked.</i>	No. 4, <i>covered.</i>	
At 7 ^h 30 ^m - - -	$50\frac{1}{8}^{\circ}$ - -	$49\frac{1}{4}^{\circ}$ - -	64°
7 45 - - -	$51\frac{1}{2}$ - -	$51\frac{1}{2}$ - -	$64\frac{1}{2}$
8 — - -	$52\frac{1}{2}$ - -	$53\frac{1}{8}$ - -	65
8 15 - - -	$53\frac{3}{4}$ - -	$54\frac{7}{8}$ - -	—
8 30 - - -	$54\frac{3}{8}$ - -	56 - -	—
8 45 - - -	$55\frac{1}{2}$ - -	$57\frac{1}{8}$ - -	—
9 — - -	$56\frac{1}{4}$ - -	$58\frac{1}{2}$ - -	—
9 30 - - -	$57\frac{1}{2}$ - -	60 - -	—
10 — - -	$58\frac{1}{4}$ - -	$61\frac{1}{4}$ - -	—
10 30 - - -	$59\frac{1}{2}$ - -	$62\frac{1}{8}$ - -	—
11 — - -	$60\frac{1}{2}$ - -	63 - -	—
11 30 - - -	61 - -	$63\frac{1}{2}$ - -	$64\frac{1}{2}$

The results of this experiment, and of several others similar to it, showed, in a manner which appeared to me to be perfectly conclusive, that those substances which part with heat with the greatest facility, or celerity, are those which also acquire it most readily, or with the greatest celerity.

If we might suppose that the temperatures of bodies are changed, not by the rays they *emit*, but by those they *receive* from other neighbouring bodies, this fact might easily be explained; but, without stopping to form any hypothesis for the explanation of these appearances, I shall proceed in my account of the various attempts I have made to elucidate, by new experiments, those parts of this interesting subject which still appeared to be enveloped in obscurity.

As the cooling of hot bodies is so much accelerated by covering their surfaces with such substances as emit calorific rays in great abundance, or with such as are much affected by the frigorific rays of the colder bodies by which they are surrounded, it seems to be highly probable, that a comparatively small part of the heat, which a body so cooled actually loses, is acquired by the air; a much greater proportion of it passing off through that *transparent* fluid, under the form of calorific rays, without affecting its temperature.

If this supposition should turn out to be well founded, the knowledge of the fact would enable us to explain several interesting phenomena, and particularly that most curious process by means of which living animals preserve an equal temperature, notwithstanding the vast quantities of heat that are continually generated in the lungs, and notwithstanding the great variations which take place in the temperature of the air in which they live.

It is evident, that the greater the power is which an animal possesses of *throwing off* heat from the surface of his body, independently of that which the surrounding air takes off, the less will his temperature be affected by the occasional changes of

temperature which take place in the air ; and the less will he be oppressed by the intense heats of hot climates.

It is well known that *negroes*, and people of colour, support the heats of Tropical climates much better than white people. Is it not probable that their *colour* may enable them to throw off calorific rays with great facility, and in great abundance ; and that it is to this circumstance they owe the advantage they possess over white people, in supporting heat ? And, even should it be true, that bodies are cooled, not in consequence of the rays they emit, but by the action of those frigorific rays they receive from other colder bodies, (which I much suspect to be the case,) yet, as it has been found by experiment, that those bodies which emit calorific rays in the greatest abundance, are also most affected by the frigorific rays of colder bodies, it is evident, that in a very hot country, where the air and all other surrounding bodies are but very little colder than the surface of the skin, those who by their colour are prepared and disposed to be cooled with the greatest facility, will be the least likely to be oppressed by the accumulation of the heat generated in them by respiration, or of that excited by the sun's rays.

With a view to throw some light on this interesting subject, I made the following experiments.

Exper. No. 27. Having covered the flat ends of both my horizontal cylindrical vessels with gold-beater's skin, I painted one of these coverings (of this animal substance) black, with Indian ink ; and then, filling both vessels with boiling hot water, I presented them, at equal distances, to the two opposite balls of the thermoscope.

The bubble of spirit of wine was immediately driven out of its

place, by the superior efficacy of the calorific rays which proceeded from the blackened animal substance.

On repeating this experiment a great number of times, and when the water in the vessels was at different degrees of temperature, (the temperature being the same in the two vessels, in each experiment,) the results uniformly indicated, that calorific rays were thrown off from the *black* surface, in greater abundance than from the equal surface which was not blackened.

Although the results of these experiments appeared to me to be so perfectly conclusive as to establish the fact in question, beyond all possibility of doubt, yet, in so interesting an enquiry, I was desirous, by varying my experiments, to bring, if possible, a variety of proofs, to support the important conclusions which result from it.

Exper. No. 28. Having covered the two large cylindrical vessels, No. 3 and No. 4, with gold-beater's skin, I painted one of them black, with Indian ink; and, filling them both with boiling hot water, I exposed them to cool, in the manner already often described, in the air of a quiet room.

No. 4, which was *blackened*, cooled through the standard interval of 10 degrees in $23\frac{1}{2}$ minutes; while the other, No. 3, which was not blackened, took up 28 minutes, in cooling through the same interval.

In a former experiment, (No. 25,) the instrument No. 4, covered with gold-beater's skin, but not blackened, had taken up $27\frac{3}{4}$ minutes, in cooling through the given interval, as we have before seen.

The results of these experiments do not stand in need of illustration; and I shall leave to physicians and physiologists to determine what advantages may be derived from a knowledge of the

facts they establish, in taking measures for the preservation of the health of Europeans who quit their native climate to inhabit hot countries.

All I will venture to say on the subject is, that were I called to inhabit a very hot country, nothing should prevent me from making the experiment of blackening my skin, or at least of wearing a black shirt, in the shade, and especially at night; in order to find out if, by those means, I could not contrive to make myself more comfortable.

Several of the savage tribes which inhabit very cold countries, besmear their skins with oil; which gives them a shining appearance. The rays of light are reflected copiously from the surface of their bodies. May not the frigorific rays, which arrive at the surface of their skin, be also reflected, by the highly polished surface of the oil with which it is covered?

If that should be the case, instead of despising these poor creatures for their attachment to a useless and loathsome habit, we should be disposed to admire their ingenuity, or rather to admire and adore the goodness of their invisible guardian and instructor, who teaches them to like, and to practice, what he knows to be useful to them.

The Hottentots besmear themselves, and cover their bodies, in a manner still more disgusting. They think themselves *fine*, when they are besmeared and dressed out according to the loathsome custom of their country. But who knows whether they may not in fact be *more comfortable*, and better able to support the excessive heats to which they are exposed? From several experiments which I made, with a view to elucidate that point, (of which an account will be given to this Society at some future period,) I have been induced to conclude, that the Hottentots

derive advantages from that practice, exactly similar to those which negroes derive from their black colour.

It cannot surely be supposed, that I could ever think of recommending seriously to polished nations, the filthy practices of these savages. That is very far indeed from being my intention; for I have ever considered cleanliness as being so indispensably necessary to comfort and happiness, that we can have no real enjoyment without it; but still, I think that a knowledge of the physical advantages which those savages derive from such practices, may enable us to acquire the same advantages, by employing more elegant means. A knowledge of the manner in which heat and cold are excited, would enable us to take measures for these important purposes with perfect certainty: in the mean time, we may derive much useful information, by a careful examination of the phenomena which occasionally fall under our observation.

If it be true, that the black colour of a negro, by rendering him more sensible to the few frigorific rays which are to be found in a very hot country, enables him to support the great heats of Tropical climates without inconvenience, it might be asked, how it happens that he is able to support, naked, the direct rays of a burning sun?

Those who have seen negroes exposed naked to the sun's rays, in hot countries, must have observed that their skins, *in that situation*, are always very shining. An oil exudes from their skin, which gives it that shining appearance; and the polished surface of that oil reflects the sun's calorific rays.

If the heat be very intense, sweat makes its appearance at the surface of the skin. This watery fluid not only reflects very

powerfully the calorific rays from the sun, which fall on its polished surface, but also, by its evaporation, generates cold.

When the sun is gone down, the sweat disappears ; the oil at the surface of the skin retires inwards ; and the skin is left in a state very favourable to the admission of those feeble frigorific rays which arrive from the neighbouring objects.

But I shall refrain from pursuing these speculations any farther at present.

I shall now proceed to give an account of several experiments, of various kinds, which were made with a view to a farther investigation of the radiations of cold bodies.

Having found, by several of the foregoing experiments, that the radiations of cold bodies affected my thermoscope very sensibly, even when placed at a considerable distance from it, and in situations where currents of cold air could not be suspected to exist, I was desirous of finding out, whether the cooling of a hot body would or would not be *sensibly* accelerated by those rays. To determine that point, I made the following experiment.

Exper. No. 29. Having provided two conical vessels, made of thin sheet brass, each $\frac{1}{4}$ inches in diameter at the base, and $\frac{1}{4}$ inches high, ending above in a cylindrical neck, 0.88 of an inch in diameter, I enclosed each of them in a cylinder of thin pasteboard, covered with gilt paper, and then covered them up with rabbit-skins, which had the hair on them, in such a manner that no part of these vessels, except their flat bottoms, was exposed naked to the air. I then covered their bottoms with gold-beater's skin, painted black with Indian ink, in order to render them as sensible as possible to calorific and frigorific rays.

This being done, I suspended these two vessels, in an erect position, or with their bottoms downwards, to the two opposite horizontal arms of a wooden stand, provided for the experiment; and I placed under each of them a pewter platter, blackened on the inside, by holding it over a lighted wax candle.

Each of these platters was 12 inches in diameter; and they were supported on the top of two shallow earthen dishes, each of which was $11\frac{1}{2}$ inches in diameter at its brim; these earthen dishes being supported on circular wooden stands, 10 inches in diameter.

A circular piece of thick drawing-paper, $12\frac{1}{2}$ inches in diameter, with a circular hole in its centre, just 6 inches in diameter, was placed on each of the platters, and served as a perforated cover to it.

The stands on which the platters were supported, were of such a height, that the upper surface of the flat bottom of each of the platters was elevated just 40 inches above the level of the floor of the room; and the horizontal arms of the wooden stand, which supported the conical vessels, was of such a height, that the flat bottoms of these vessels (which were placed perpendicularly over the centres of the platters) were just 4 inches above the flat horizontal surface of the bottoms of the platters.

One of the platters was at the temperature of the air of the room, (63° F.) but the other was kept constantly ice-cold, during the whole of the time the experiment lasted, by means of pounded ice and water, which was put into the earthen dish, over which, or rather in which, this platter was placed.

Each of the platters was just 1 inch deep, measured from the level of the top of its brim to the level of the upper surface of

the flat part of its bottom : this flat part was about 8 inches in diameter.

The two conical vessels were now filled with boiling hot water, and the times of their cooling were carefully observed.

From the above description of the apparatus used in this experiment, it is evident, that the vessel which was suspended over the ice, could not be reached by any streams of cold air that might be occasioned by that ice, or by the cooled sides of the vessel which contained it ; for the air which, coming into contact with the sides of that vessel, was cooled by it, becoming specifically heavier than it was before, naturally descended, and spread itself out on the floor of the room ; and the perforated circular sheet of paper, which was laid down horizontally on the platter, effectually prevented any of the air so cooled from being thrown upwards against the bottom of the conical vessel, (placed immediately over the platter,) by any occasional undulation of the air in the room.

To preserve the air of the room in a state of perfect quietness, not only the doors and windows, but even the window-shutters of the room, were kept shut ; so much light only being admitted occasionally, as was necessary to observe the thermometers which were placed in the conical vessels.

In order to guard still more effectually the bottoms of the vessels which were cooling, from the effects of occasional undulations in the air of the room, over each of these vessels there was drawn a cylindrical covering of very fine thin post paper ; the lower open end of which projected just half an inch below the horizontal level of the flat bottom of the vessel. These cylindrical coverings of post paper were made to fit, as exactly as

possible, the cylinders of pasteboard by which the sides of the conical vessels were covered, and defended from the air; and the warm coverings of fur (rabbit-skins) were put over all.

To confine the heat still more effectually, a quantity of eider-down had been introduced between the outside of each conical vessel, and its cylindrical neck, and the inside of the hollow cylinder of pasteboard in the axis of which it was fixed and confined.

The result of this experiment was very conclusive. The conical vessel which was suspended over the *ice-cold* pewter platter, cooled through the standard interval of 10 degrees, (namely, from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room,) in 33 minutes and 42 seconds; whereas, the other vessel, which was not over ice, required 39 minutes and 15 seconds, to cool through the same interval.

Exper. No. 30. On repeating this experiment the next day, the air of the room still remaining at 63°, the times of cooling through the given interval were as follows.

Min. Sec.

The vessel suspended over the ice-cold platter, in 33 15

The other vessel, in - - - - - 39 30

From the results of these experiments (which were made with the greatest possible care) it appears, that the radiations of cold bodies act on warmer bodies, *at a distance*, and gradually diminish their temperatures.

It will likewise be evident, when we consider the matter with attention, that the cooling of the vessel which was suspended over the ice-cold platter, was in fact considerably more accelerated by the frigorific radiations from that cold surface than it appears to have been, when we estimate the effects produced

simply by the difference of the times taken up in the cooling of the two vessels, without having regard to any other circumstance.

These times are, no doubt, inversely as the velocities of cooling; but, as all the heat lost by the vessels, during the time of their cooling, did not pass off through their flat bottoms, and as the rays from the cold surface fell on the *bottom only* of the vessel which was suspended over it, without at all affecting its covered sides, the velocity with which the heat made its way through the covered sides of the vessels was the same in both; consequently, more heat must have passed that way, and of course less through the bottom of the vessel, when the time of cooling was the longest, that is to say, in the vessel which was not placed over ice.

As the cooling of these vessels is a complicated process, I will endeavour to elucidate the subject still farther.

As the two conical vessels were of the same form and dimensions, and contained equal quantities of hot water, the quantities of heat they parted with, in being cooled the same number of degrees, must of course have been equal.

Expressing that quantity by the algebraic symbol a , and putting $x =$ the quantity of heat which passed off through the covered sides of the vessel which was suspended over ice, during the time it was cooling through the given interval of 10 degrees, and $y =$ the quantity which passed off through the covered sides of the other vessel, during the time that vessel was cooling through the same interval; the quantity of heat which passed off through the bottom of the vessel which was placed over ice, during the time it was cooling through the given interval, must

have been $= a - x$; and that which passed off through the bottom of the other vessel, during the time of its cooling through the same interval, $= a - y$.

But, as the velocities of the heat through the covered sides of both vessels must have been equal, the quantities of heat which passed off *that way* must have been as the times of cooling.

The times of cooling in the last mentioned experiment, (No. 30,) were as follows :

	Min.	Sec.	Seconds.
Of the vessel suspended over ice	33	15	$= 1995$
Of the other vessel - - -	39	30	$= 2370$

x is therefore to y , as 1995 to 2370 ;

consequently, $x = \frac{1995y}{2370} = 0.84177y$;

And, substituting for x , its value $= 0.84177y$, the quantities of heat which passed off through the bottoms of the two vessels, in the experiment in question, (No. 30,) must have been $= a - 0.84177y$, for the vessel which was suspended over ice ; and $= a - y$, for the other vessel.

And, as y is greater than $0.84177y$, consequently $a - 0.84177y$ is greater than $a - y$, or the quantity of heat which passed off *through the bottom* of the vessel which was cooled the most rapidly, was greater than that which passed off *through the bottom* of the other vessel ; and hence we perceive, that the effect produced by the frigorific rays from the cold surface, in the experiments in question, was *greater* than it appeared to be, at first sight, when it was estimated by the times of cooling.

To determine exactly *how much* the cooling was accelerated by the presence of the cold body, it is necessary to find out how much heat actually passed off through the bottoms of the two vessels, in the experiments in question. This we will endeavour

to do, by comparing the results of those experiments, with the results of some other experiments of a similar nature.

In the experiment No. 28, a cylindrical vessel of thin sheet brass, $\frac{1}{4}$ inches in diameter, and $\frac{1}{4}$ inches in height, covered with gold-beater's skin painted black with Indian ink, being filled with hot water, and exposed to cool in the air of a large quiet room, cooled from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room, in $23\frac{1}{2}$ minutes.

The quantity of surface by which this vessel was exposed to the cold air, was $= 74.5581$ superficial inches, exclusive of its neck, which was well covered up with fur.

The quantity of surface which was exposed to the air, in the foregoing experiments with the conical vessels, or the area of the bottom of each of the vessels, was $(\frac{1}{4} \times 3.14159) = 12.4263$ superficial inches.

As the diameters and heights of the conical and cylindrical vessels were equal, the contents of the former must have been to the contents of the latter as 1 to 3; and the quantities of heat which they lost in cooling were as their contents.

If now the cylindrical vessel lost a quantity of heat $= 3$, in $23\frac{1}{2}$ minutes, it would have disposed of a quantity $= 1$, (equal to that which the conical vessel lost,) in one-third part of that time, or in 7 minutes and 50 seconds.

But the quantity of surface exposed to the air in the experiment with the cylindrical vessel, was to that so exposed in the experiment with the conical vessel, as 74.5581 to 12.4263, or as 6 to 1.

Now, as the time in which any given quantity of heat can pass out of any closed vessel, into, or through, any cold fluid medium by which the vessel is surrounded, must be inversely as the

surface of the vessel, other things being equal; if a quantity of heat $= 1$ could pass out of the cylindrical vessel in 7 minutes and 50 seconds, it would require 6 times as long, or 47 minutes, to pass out of the conical vessel, *through its flat bottom*, supposing no heat whatever to escape through the covered sides of that vessel.

If now the whole of the heat which the conical vessel actually lost, would have required 47 minutes to have passed through the bottom of that vessel, it is evident that the quantity which actually passed through that surface, in the experiment in question, (No. 30,) could not have been, to the whole quantity actually lost, in a greater proportion than that of the times, or as $39\frac{1}{2}$ to 47.

Assuming any given number, as 10000, for instance, to represent the whole of the heat lost in the experiment, we can now determine what part or proportion of it passed off through the bottom of the conical vessel, and consequently how much of it must have made its way through its covered sides.

If the whole quantity, $= 10000$, would have required 47 minutes to have passed through the bottom of the vessel, the quantity which actually passed through that surface in $39\frac{1}{2}$ minutes, could not possibly have amounted to more than 8404, $= a - y$.

For it is 47 min. to 10000, as $39\frac{1}{2}$ min. to 8404. The remainder of the heat, $= 10000 - 8404 = 1396$ parts, ($= y$,) must have made its way through the covered sides of the vessel.

And, if a quantity of heat $= 1396$, required $39\frac{1}{2}$ minutes to make its way through the covered sides of one of the conical vessels, the quantity which made its way through the covered sides of the other in $33\frac{1}{4}$ minutes, could not have amounted to

more than 1175 parts; and the remainder of that which was actually disposed of in the experiment, $= 10000 - 1175 = 8825$, ($= a - x$,) must have passed off through the bottom of the instrument.

Hence it appears, that the quantity of heat which actually passed off through the bottom of the conical vessel which was placed over ice, in $33\frac{1}{4}$ minutes, was to that which passed off in $39\frac{1}{2}$ minutes, through the bottom of the other vessel, as 8825 to 8404; and consequently, that the velocity with which the heat passed through the bottom of the vessel which was exposed to the frigorific rays from the surface of the cold platter, was to the velocity with which it passed through the bottom of the other vessel, in the compound ratio of 8825 to 8404, and of $39\frac{1}{2}$ to $33\frac{1}{4}$; or as 10000 to 8025, which is as 5 to 4, very nearly.

From these experiments and computations it appears, that the cooling of the hot body which was placed over the ice-cold platter, was sensibly, and very considerably, accelerated by the vicinity of that cold body; may we not venture to say,—by the frigorific rays which proceeded from it?

I made several other experiments, similar to those just described, and with similar results; but I shall not take up the time of the Society, by giving a detailed account of them. I may perhaps, at a future time, find occasion to mention some of them more particularly.

In the two last-mentioned experiments, as the conical vessels were suspended in an erect position, and had a circular band or hoop of fine post paper, by which the lower end of each of them was surrounded, and which projected downwards half an inch below the horizontal level of the bottom of the vessel, and as the air which came into immediate contact with the bottom of the

vessel, and received heat from it, (though it became specifically lighter than it was before,) could not make its escape *upwards*, into the atmosphere, being confined and prevented from moving upwards by the thin projecting hoop of paper, there is no doubt but that the time of cooling was prolonged by this arrangement; for, there being much reason to believe that the propagation of heat downwards, in air, from one particle of that fluid to another, is either quite impossible, or so extremely slow as to be imperceptible, as a succession of fresh particles of cold air was prevented from coming into contact with the bottoms of the vessels, but very little heat could have been given off *immediately* to the air in those experiments.

In order to be able to form some probable conjecture respecting the quantity so given off, in cases where the succession of fresh particles of air is free and uninterrupted, I made the following experiment.

Exper. No. 31. The two conical vessels used in the last experiment, (which I shall now distinguish by calling the one No. 5, and the other No. 6,) being left suspended in the air, to the two horizontal arms of their wooden stand, at the height of 44 inches above the floor of the room, (the pewter platters, the earthen dishes, and the stands on which they were placed being removed,) both the vessels were again filled with boiling hot water, and exposed to cool in the air.

The vessel No. 5 remained in a vertical position, or with its flat bottom in a horizontal position, as before; but the vessel No. 6 was now reclined, so that its axis, and consequently the plane of its flat bottom, made an angle with the plane of the horizon, of 45 degrees. In this position of the vessel No. 6, it is evident that the air, heated by coming into contact with its

bottom, had full liberty to escape *upwards*, and to make way for other particles of colder air to come into contact with the hot surface, and be heated, rarefied, and forced upwards, in their turns; and, under these circumstances, it might reasonably be expected, that as much heat as possible would be communicated *immediately* to the air, by the hot body; and that the heat so communicated would of course accelerate the cooling of that vessel.

It was in fact cooled in a shorter time than the other, No. 5, which was suspended in a vertical position; but the difference of the times of cooling was very small; which indicates, if I am not mistaken, that a comparatively small quantity of the heat a hot body loses, when it is cooled in air, is communicated to that fluid; much the greater part of it being sent off through the air, to a distance, in calorific rays.

The vessel No. 5 was found to cool through the standard interval of 10 degrees in $38\frac{1}{2}$ minutes; and No. 6, which was in a reclined position, in $37\frac{1}{4}$ minutes.

It will no doubt be remarked, that the vessel No. 5 cooled somewhat faster in this experiment than it had done in the two preceding experiments, (No. 29 and No. 30,) when it stood over a pewter platter, which (at the beginning of the experiment at least) was at the same temperature as the air of the room.

The calorific rays from the bottom of the vessel, heating the platter in some small degree, and still more perhaps the upper surface of the perforated sheet of paper which covered it, the frigorific rays from these bodies, were, on that account, somewhat less powerful in lowering the temperature of the neighbouring hot body; and the time of its cooling was consequently a little prolonged.

In one of the preceding experiments, it cooled through the given interval in $39\frac{1}{2}$ minutes, and in the other in $39\frac{1}{4}$ minutes; but, in this experiment, it took up only $38\frac{1}{2}$ minutes, in cooling through it, as we have just seen.

Supposing now, (what appears to me to be not improbable,) that all, or very nearly all, the heat lost by the instrument No. 5 passed off in rays *through* the air, we can ascertain what part of the heat lost by the instrument No. 6, was communicated *to the air* which came into contact with its surface.

Putting the total quantity of heat lost by each of the instruments, in cooling through the given interval, = 10000; as we have just seen that a quantity of heat = 1396, passes through the covered sides of each of these instruments in $39\frac{1}{2}$ minutes, the quantities so lost in this experiment must have been as follows. By the instrument No. 5, in $38\frac{1}{2}$ minutes, = 1081; by No. 6, in $37\frac{1}{4}$ minutes, = 1046; and, deducting these quantities so lost (through the covered sides of the instruments) from the total quantity lost by each, (= 10000,) we shall find out how much heat passed off *through the bottom* of each of the instruments.

For the instrument No. 5, it is $10000 - 1.081 = 9199$,

And for - - No. 6, $10000 - 1.046 = 9954$.

If now the whole of the heat lost through the bottom of the instrument No. 5, passed off *through* the air in rays, as there is no reason to suppose that a less quantity passed off in the same time, *in the same way*, through the bottom of the instrument No. 6, it appears, that this last mentioned instrument must have lost, *by radiation*, or in rays which passed *through* the air, a quantity of heat = 9597.

For it is $38\frac{1}{2}$ minutes to 9919, as $37\frac{1}{4}$ minutes to 9597.

And, if of the total quantity of heat which passed off through the bottom of the conical instrument No. 6, = 9954, a quantity = 9597 passed off *through* the air, in calorific rays, the remainder only, (9954 - 9597,) which amounts to no more than 357 parts, could have been communicated to the air.

Hence it would appear, that when a hot body is cooled in air, $\frac{1}{27}$ part only of the heat which it loses is acquired by the air; for 357 is to 9597, as 1 to 27, very nearly. But I shall refrain from enlarging farther on this subject at present.

One of the objects which I had in view, in the last experiment, was, to find out whether the cooling of a hot body in air, is or is not sensibly accelerated, or retarded, by the greater or lesser distance at which the body is placed from other neighbouring solid bodies, when these neighbouring bodies are at the same temperature as the air; and, as a comparison of the result of this experiment, with the results of the two preceding experiments, so strongly indicated that the cooling of the conical vessel, in the preceding experiments, had in fact been *retarded* by the vicinity of the pewter platter over which it was suspended, I was now induced to repeat these experiments with some variations.

These investigations appeared to me to be of the more importance, as I conceived that the results of them might lead to a discovery of one of the causes of the warmth of clothing.

Exper. No. 32. I now placed the pewter platters once more in their former stations, perpendicularly under the bottoms of the two conical vessels, but at the distance of 3 inches only; that which was under the vessel No. 5 being at the temperature of the air of the room, (62°,) while that placed under the vessel

No. 6, was kept ice-cold, by means of pounded ice and water, which was put into the earthen dish on the brim of which it was supported.

The times of the cooling of the vessels, through the standard interval of 10 degrees, were as follows.

No. 5 - - - in $40\frac{1}{4}$ minutes.

No. 6, which was over ice, in $33\frac{1}{4}$

Exper. No. 33. I repeated this experiment once more, but varied it, by bringing the pewter platters still nearer to the bottoms of the conical vessels. The flat horizontal part of each of the platters, was now only 2 inches below the flat surface of the bottom of the conical vessel which was suspended over it. Both the platters still remained covered by their flat circular perforated covers of paper; but it should be remembered, that the circular hole in the centre of each of these covers was no less than 6 inches in diameter, and consequently, that a large portion of the flat part of the bottom of the platter was in full view (if I may use that expression) of the bottom of the vessel which was suspended over it.

The times of cooling, in this experiment, were as follows.

No. 5 cooled through the given interval in $42\frac{3}{4}$ minutes.

No. 6, which was over ice - - - in $32\frac{1}{2}$ minutes.

The results of these experiments show, (what indeed might have been expected, especially on a supposition that the heating and cooling of bodies is effected by means of radiations,) that although the cooling of the hot body suspended over a surface kept constantly cold by artificial means, was accelerated by being brought nearer to that cold surface, yet, in a case where the cold surface was less intensely cold, and where its tempera-

ture could be sensibly raised by the calorific rays from the hot body, the cooling of the hot body was retarded by a nearer approach of that cold surface.

From the results of these experiments we may safely conclude, that if the hot body, instead of being a conical vessel covered up on all sides except its flat bottom, had been a globe, and if this hot globe had been suspended in the centre of another larger thin hollow sphere, (this last being, at the beginning of the experiment, at the same temperature as the air and walls of the room,) the vicinity of the surface of this hollow globe, to the surface of the hot body, would have retarded the cooling of the hot body, in the same manner as the cooling of the conical vessel No. 5 was retarded in the foregoing experiments; and if, instead of inclosing the hot body in the centre of a single hollow sphere, of any given thickness, it were placed in the common centre of a number of much thinner concentric spheres, of different diameters, the time of cooling would be still more retarded.

By tracing the various operations which would take place in the cooling of the hot body, in this imaginary experiment, we shall become acquainted with the nature of those which actually take place, when the cooling of a hot body is prolonged by means of warm clothing.

From the results of several of the foregoing experiments we may conclude, that, supposing the thin concentric hollow spheres in which the hot body is confined to be made of metal, the cooling will be slower, if the surfaces of these spheres are polished, than if they are unpolished, or blackened: and hence we might very naturally be led to suspect, (what is probably true in fact,) that the *warmth* of any kind of substance used as

clothing, or its power of preventing our bodies from being cooled by the influence (frigorific radiations) of surrounding colder bodies, depends very much *on the polish of its surface*.

If, with the assistance of a microscope, we examine those substances which supply us with the warmest coverings, such for instance as furs, feathers, silk, &c. we shall find their surfaces not only smooth, but also very highly polished; we shall also find that, other circumstances being equal, those substances are the warmest which are the finest, or which are composed of the greatest number of fine polished detached threads or fibres.

The fine white shining fur of a Russian hare, is much warmer than coarse hair; and fine silk, as spun by the silk-worm, is warmer than the same silk twisted together into coarse threads; as I found by actual experiments, an account of which has already been laid before this Society, and published in the Philosophical Transactions.

I formerly considered the warmth of natural and artificial clothing, as depending *principally* on the obstacle it opposes to the motions of the cold air by which the hot body is surrounded; but, by a patient and careful examination of the subject, I have been convinced, that the efficacy of radiation is much greater than I had supposed it to be.

From the result of the experiment No. 31, we might be led to conclude, that a very small part only of the heat which a hot body appears to lose when it is cooled in air, is in fact communicated to that fluid; a much greater portion of it being communicated to other surrounding bodies at a distance; and, in one of my former experiments, a hot body was cooled, though it was placed in a TORRICELLIAN vacuum.

These researches appear to me to be the more interesting, as I have long been of opinion, that it must be by experiments of this kind, (showing in what manner the temperature of bodies are affected reciprocally, at different degrees of temperature, and at different distances,) that the hypothesis of radiation must be established, or proved to be unfounded.

When I speak of heat as being communicated to air *immediately* by a hot body which is cooled in it, I mean only, that it is not first communicated to other neighbouring bodies, and then given *by them* to the particles of air with which they happen to be in contact. In this last mentioned way, much of the heat, no doubt, which a hot body loses when cooled in air, is ultimately communicated to that fluid.

I am far from supposing that the particles of air which, coming into contact with a hot body, are heated in consequence of that near approximation, receive heat in any other *manner* than that in which other bodies, at a greater distance, receive it. If, in the one case, it be generated, or excited, by the agency of calorific rays, or undulations, caused by the hot body, it must, I am persuaded, be excited in the same manner in the other.

The reason why the particle of air which is in immediate contact with a hot body is heated, while other particles, near it, are not affected by the calorific rays from the hot body, which are continually passing by them, through the air, is, I conceive, because the particle heated is at *the surface of the fluid*, (air,) where these rays are either reflected, refracted, or absorbed; but, when a ray has once passed the surface of a transparent fluid, it proceeds straight forwards, without being farther affected by it, *and consequently without affecting it*, till it comes to the confines of the medium, or to the surface of some other body.

If this hypothesis of the communication, or rather *generation*, of heat, and of cold, by radiation, be true, it will enable us to explain, in a satisfactory manner, what has been called the *non-conducting power* of transparent fluids, with respect to heat; for, if heat be really communicated, or excited, in the manner above described, it is quite evident that a *perfectly transparent fluid* can receive heat only at its surface; and consequently, that heat cannot be propagated in such a fluid, by communication, from one particle of the fluid to another.

By a *transparent fluid*, I mean such an one as admits the calorific and frigorific rays, emitted by hot and by cold bodies, to pass freely through it, without obstructing their passage, or diminishing their intensities.

Whether any of the fluids with which we are acquainted be *perfectly* transparent in this sense of the word, or not, I will not pretend to say; but there is reason to think that pure water, and air, and most other fluids which are transparent to light, possess a high degree of transparency, in regard to calorific and frigorific rays; or that they give a very free passage to them, when they have once passed their surfaces.

An even or polished surface has been found to facilitate very much the reflection of the rays of light. May it not, in all cases, have an equal tendency to facilitate the reflection of calorific and frigorific rays?

In the experiments with the large cylindrical vessels, where they were exposed *naked* to cool in the air, their surfaces were polished, and they were a long time in cooling. But, when the surface of the vessel was blackened, or covered with other substances, the vessel was found to cool much more rapidly.

A large proportion of the frigorific rays from the surrounding

colder bodies were, in the former case, reflected at the polished surface of the metallic vessel; but, in the latter case, more of them were absorbed.

When a large drop of water rolls about, without being evaporated, upon the flat surface of a piece of red-hot iron, the surface of the drop is *polished*; and, the calorific rays being mostly reflected, the water is very little heated, notwithstanding the extreme intensity of the heat of the iron, and its nearness to the water.

If the iron be *less hot*, the water penetrates the pores of the oxide which covers the metal,—the drop ceases to have a polished surface,—acquires heat very rapidly,—and is soon evaporated.

If a drop of water be placed on the clean and polished surface of a metal not so easily oxidable as iron, it will retain its spherical form and polished surface, under a lower degree of temperature than on iron; and consequently will be less heated, and less rapidly evaporated by a moderate heat.

If a large drop of water be put carefully into a clean silver spoon, previously heated very hot, (that is to say, so hot as to give a loud hissing noise when touched with the wetted finger, but much below the heat of red-hot metal,) the drop will support, or rather *resist*, this heat for a considerable time; but, after the spoon has been suffered to cool down nearly to the temperature of boiling water, a drop of water put into it will be evaporated instantaneously.

It appears, from the results of these experiments, to be probable that, under high temperatures, air is attracted by metals so much more strongly than water, that even the weight of a drop of water is not sufficient to force away the stratum of air

which covers, and adheres to, the surface of a metal on which the drop reposes ; but, at lower temperatures, this does not seem to be the case.

The following experiment, which I made several months ago, with a view to investigate the cause of the slow evaporation of drops of water placed on hot metals, will, I think, throw much light on this subject.

Exper. No. 34. Taking a clean polished silver spoon, I blackened the inside of it, by holding it over the flame of a wax candle ; then, putting a large drop of water into it, I found, as I expected, that the drop took a spherical form, and rolled about in the spoon, without wetting its blackened surface.

I now held the spoon over the flame of a candle, and attempted to make the water boil ; but I found it to be absolutely impossible. The handle of the spoon became so very hot, that I could not hold it in my hand without being burnt, though it was wrapped up in three or four thicknesses of linen ; but still the drop of water did not appear to be at all affected by this intense heat. If the bowl of the spoon were touched with the finger, a hissing noise announced that it was extremely hot ; but still, the water remained perfectly quiet in the spoon, without being evaporated.

Having in vain attempted to make this drop of water boil, and not being able to hold the spoon over the flame of the candle any longer, on account of the heat of its handle, I now poured the drop into the palm of my hand. I found it to be warm, but by no means scalding hot.

By holding the spoon, with a pair of tongs, over the flame of the candle for a longer time, I found that a drop of water in

the spoon gradually *changed its form*, became less, and was at length evaporated: from being spherical and lucid, it gradually took an oblong form, and its surface became obscure; and, when it was evaporated, it left a kind of skin behind it, which was evidently composed of the particles of black matter, which had by degrees attached themselves to its surface, and which probably had contributed not a little to its being at last heated, and evaporated.

The change in the form of the drop of water, and more especially the gradual loss of its lucid appearance, made me suspect that it had turned round during the experiment. If it really did so, its motion must either have been extremely rapid, or very slow; for, though I examined it with great attention, I could not perceive that it had any rotatory motion.

I will take the liberty to mention another little experiment, which I have often made, to amuse myself and others, though it may perhaps be thought too trifling to deserve the attention of the Royal Society.

Exper. No. 35. If a large drop of water be formed at the end of a small splinter of light wood, (deal, for instance,) and this drop be thrust quickly into the centre of the flame of a newly snuffed candle, which burns bright and clear, the drop of water will remain for a considerable time in the centre of the flame, and surrounded by it on every side, without being made to boil, or otherwise apparently affected by the heat; and, if it be taken out of the flame, and put upon the hand, it will not be found to be scalding hot.

If it be held for some time in the flame, it will be gradually diminished, by evaporation; but there is much reason to think,

that the heat which it acquires is not communicated to it by the flame, but by the wood to which it adheres, which is soon heated by the flame, and even set on fire.

I cannot refrain from just observing, that it appears to me to be extremely difficult to reconcile the results of any of the foregoing experiments, with the hypothesis of modern chemists respecting the *materiality of heat*.

Deeply sensible of the insufficiency of the powers of the human mind, to unfold the mysteries of nature, and discover the agents she employs, and their mode of action, in her secret and invisible operations; and being moreover fully aware of the danger of forming an attachment to a false theory, and of the folly of wasting time in idle speculations; I have ever, in my philosophical researches, been much more anxious to discover new facts, and to show how the discoveries of others may be made useful to mankind, than to invent plausible theories; which much oftener tend to misguide, than to lead us in the path of truth and science.

There are however situations, in which an experimental enquirer sometimes finds himself, where it is almost impossible for him to abstain from forming, or adopting, some general theory, for the purpose of explaining the phenomena which fall under his observation, and directing him in his future researches.

Finding myself in that situation at this time, I beg the attention, and above all the *indulgence*, of the Society, while I endeavour to explain the conjectures I have formed, respecting the nature of heat, and the mode of its communication.

Hot and *cold*, like *fast* and *slow*, are mere relative terms; and, as there is no relation, or proportion, between motion and a state of rest, so there can be no relation between any degree

of heat and absolute cold, or a total privation of heat: hence it is evident, that all attempts to determine the place of *absolute cold*, on the scale of a thermometer, must be nugatory.

It seems probable that *motion* is an essential quality of matter; and that rest is no where to be found in the universe.

We well know, that all those bodies which fall under the cognizance of our senses are in motion; and there are many appearances which seem to indicate, that the constituent particles of all bodies are also impressed with continual motions among themselves; and that it is these motions (which are capable of augmentation and diminution) that constitute the *heat* or temperature of sensible bodies.

The only effects of which we have any idea, resulting from the action of one body on another, are a change of velocity, or a change of direction, or both. We perceive, it is true, that certain bodies have a power of affecting certain other bodies *at a distance*; but this is no proof that the effects produced are essentially different from those which result from collision; for, if an elastic body be interposed between the two bodies, their actions on each other may be communicated through such intermediate elastic body, which, when the action is at an end, and the effects resulting from it on the two bodies have taken place, will be in the same state precisely in which it was before the action began.

If a bell, or any other solid body, *perfectly elastic*, placed in a perfectly elastic fluid, and surrounded by other perfectly elastic solid bodies, were struck, and made to vibrate, its vibrations would, by degrees, be communicated, by means of the undulations, or pulsations they would occasion in the elastic fluid medium, to the other surrounding solid and elastic bodies. If

these surrounding bodies should happen to be already vibrating, and with the same velocity as that with which the bell is made to vibrate by the blow, the undulations in the elastic fluid, occasioned by the bell, would neither increase nor diminish the velocity or frequency of the vibrations of the surrounding bodies ; neither would the undulations caused by the vibrations of these bodies tend to accelerate, or to retard, the vibrations of the bell. But, if the vibrations of the bell were more frequent than those of the surrounding bodies, the undulations it would occasion in the elastic fluid, would tend to accelerate the vibrations of the surrounding bodies : on the other hand, the undulations occasioned by the slower vibrations of the surrounding bodies, would retard the vibrations of the bell ; and the bell, and the surrounding bodies, would continue to affect each other, until, by the vibrations of the latter being gradually increased, and those of the former diminished, in consequence of their actions on each other, they would all be reduced to the same *tone*.

Supposing now, that heat be nothing more than the motions of the constituent particles of bodies among themselves, (an hypothesis of ancient date, and which always appeared to me to be very probable,) if for the bell we substitute a hot body, the cooling of it will be attended by a series of actions and reactions, exactly similar to those just described.

The rapid undulations occasioned in the surrounding ethereal fluid, by the swift vibrations of the hot body, will act as calorific rays on the neighbouring colder solid bodies ; and the slower undulations, occasioned by the vibrations of those colder bodies, will act as frigorific rays on the hot body ; and these reciprocal actions will continue, but with decreasing intensity, till the hot body, and those colder bodies which surround it, shall, in

consequence of these actions, have acquired the same temperature, or until their vibrations have become isochronous.

According to this hypothesis, *cold* can with no more propriety be considered as the absence of *heat*, than a low or grave sound can be considered as the absence of a higher or more acute note; and the admission of rays which generate cold, involves no absurdity, and creates no confusion of ideas.

On a superficial view of the subject, it may perhaps appear difficult to reconcile solidity, hardness, and elasticity, with those never-ceasing motions which we have supposed to exist among the constituent particles of all bodies; but a patient investigation of the matter will show, that the admission of that supposed fact, instead of rendering it more difficult to form distinct and satisfactory ideas of the causes on which those qualities of bodies depend, will rather facilitate those abstruse researches.

Judging from all the operations of nature, of the causes of which we are able to form any distinct ideas, we are certainly led to conclude, that the force of dead matter, (and perhaps of living matter also,) or its power of affecting, that is to say, of *moving*, other matter, or of *resisting its impulse*, depends on its motion.

If, therefore, solid (or fluid) bodies have any powers whatever, either of impulse or of resistance, it appears to me to be more reasonable to ascribe them to the living forces residing in them, —to the never-ceasing motions of their constituent particles,—than to suppose them to be derived from their want of power, and their total indifference to motion and to rest.

No reasonable objection against this hypothesis, (of the incessant motions of the constituent particles of all bodies,) founded on a supposition that there is not room sufficient for these

motions, can be advanced; for we have abundant reason to conclude, that if there be in fact any indivisible solid particles of matter, (which however is very problematical,) these particles must be so extremely small, compared to the spaces they occupy, that there must be ample room for all kinds of motions among them.

And, whatever the nature or directions of these internal motions may be, among the constituent particles of a solid body, as long as these constituent particles, in their motions, do not break loose from the systems to which they belong, (and to which they are attached by gravitation,) and run wild in the vast void by which each system is bounded, (which, as long as the known laws of nature exist, is no doubt impossible,) the form or external appearance of the solid cannot be sensibly changed by them.

But, if the motions of the constituent particles of any solid body be either increased or diminished, in consequence of the actions, or radiations, of other distant bodies, this event could not happen without producing some visible change in the solid body.

If the motions of its constituent particles were *diminished* by these radiations, it seems reasonable to conclude, that their elongations would become less, and consequently, that the volume of the body would be contracted; but, if the motions of these particles were increased, we might conclude, *a priori*, that the volume of the body would be expanded.

We have not sufficient data to enable us to form distinct ideas of the nature of the change which takes place when a solid body is melted; but, as fusion is occasioned by heat, that is to say, by

an augmentation (from without) of that action which occasions expansion, if expansion be occasioned by an increase of the motions of the constituent particles of the body, it is, no doubt, a certain additional increase of those motions, which causes the form of the body to be changed; and, from a solid, to become a fluid substance.

As long as the constituent particles of a solid body which are at the surface of that body, do not, in their motions, *pass by each other*, the body must necessarily retain its form or shape, however rapid those motions or vibrations may be; but, as soon as the motion of these particles is so augmented that they can no longer be restrained, or retained within these limits, the regular distribution of the particles, which they acquired in crystallization, is gradually destroyed; and the particles so detached from the solid mass, form new and independent systems, and become a liquid substance.

Whatever may be the figures of the orbits which the particles of a liquid describe, the mean distances of those particles from each other remain nearly the same as when they constituted a solid, as appears by the small change of specific gravity which takes place, when a solid is melted, and becomes a liquid; and, on a supposition that their motions are regulated by the same laws which regulate the solar system, it is evident that the additional motion they must necessarily acquire, in order to their taking the fluid form, cannot be lost, but must continue to reside in the liquid, and must again make its appearance, when the liquid changes its form, and becomes a solid.

It is well known that a certain quantity of *heat* is requisite to melt a solid; which quantity disappears, or remains *latent* in the

liquid produced in that process; and that the same quantity of heat reappears, when this liquid is congealed, and becomes a solid body.

But, before I proceed any farther in these abstruse speculations, I shall endeavour to investigate some of the consequences which would necessarily result from the radiations of hot and of cold bodies, supposing those radiations to exist, and their motions and actions to be regulated by certain assumed laws.

And first, it is evident that the intensity of the rays emitted by a luminous point, in a perfectly transparent medium, is every where as the squares of the distance from that point, inversely; for the intensity of those rays must be as their condensation; and their condensation being diminished, in proportion as the space they occupy is increased, if we suppose all the rays which proceed in all directions from any point, to set out at the same instant, and to move with the same velocity, in right lines, these simultaneous rays (or undulations) will, in their progress, form a sphere, which sphere will increase continually in size, as the rays advance; and, as all the rays must be found at the surface of this sphere, their intensity, or condensation, must necessarily be as the surface of the sphere, inversely, or as the squares of the distance, inversely, from the centre of the sphere, or, which is the same thing, from the luminous point from which these rays proceed; the surfaces of spheres being to each other as the squares of their radii.

Supposing now, (what indeed appears to be incontrovertible,) that the intensity of the rays which hot and cold bodies emit, in a medium perfectly transparent, follows the same law, we can determine what effects must be produced, by the largeness, or

smallness, of the confined space (of a room, for instance) in which a hot body is placed, to cool.

To simplify this investigation, we will suppose this confined space to be a hollow sphere of ice, 9 feet in diameter, at the temperature of freezing water; and the hot body to be a solid sphere of metal, 2 inches in diameter, at the temperature of boiling water, placed in the centre of it; and we will suppose farther, that this hollow sphere is void of air, and that the cooling of the hot body is effected solely by the frigorific rays from the ice.

The question to be determined is, in what manner the cooling of the hot body would be affected, by increasing the diameter of this hollow sphere of ice?

Let us suppose its diameter to be increased to 18 feet. Its internal surface will then be to the surface of a sphere 9 feet in diameter, as the square of 18 to the square of 9, that is to say, as 324 to 81, or as 4 to 1. And, as the quantity of frigorific rays emitted are, *cæteris paribus*, as the surface from which they proceed, the quantity of rays emitted by the internal surface of the larger sphere, will be to the quantity emitted by the internal surface of the smaller, as 4 to 1.

But the intensities of these rays, at the common centre of these spheres, (where the hot body is placed,) being as the squares of the distances from the radiating points, inversely, the intensity of the rays from the internal surface of the smaller sphere, must be to the intensity of the rays from the internal surface of the larger sphere, as 4 to 1, at the common centre of those spheres.

Now, as the time of the cooling of the hot body will depend

on the *quantity* of frigorific rays which arrive at its surface, and on the *intensity* of their action; and, as the intensity of the rays from the internal surface of the sphere, at its centre, is diminished in the same proportion as the surface of the sphere is augmented when its diameter is increased; it follows, that a hot body placed in the centre of a hollow sphere, at any given constant temperature below that of the hot body, will be cooled in the same time, or with the same celerity, whatever may be the size of the sphere.

If this conclusion be well founded, (and I see no reason to suspect that it is not so,) it will follow, from the principles assumed, that the hot body will be cooled in the same time, in whatever part of the hollow sphere it be situated. And, as the cooling of the body is not affected, that is to say, accelerated, or retarded, either by the greater or smaller size of the inclosed space in which it is confined, or by its situation in that confined space, so it cannot be in any manner affected, either by the form of that hollow space, or by the presence of a greater or less number of other solid bodies; provided always, that all these surrounding bodies be at the same constant temperature.

If, however, any of these surrounding bodies, the temperature of which is liable to be sensibly changed during the experiment, by the calorific rays emitted by the hot body, be placed *very near* that body, the cooling of that hot body will be retarded; the rays from this neighbouring body, *so heated*, being less frigorific than those from other bodies at a greater distance, which it intercepts.

The results of all my experiments on the cooling of bodies, tended uniformly to confirm the above conclusions.

Admitting that the cooling of a hot body is effected solely by

the rays which proceed from colder bodies, and that these rays, like those of light, are reflected, refracted, and concentrated, according to certain known laws, by the polished surfaces of mirrors and lenses, it might perhaps be imagined, that the cooling of a hot body might be accelerated, or retarded, by giving it some peculiar form; or by placing near it, and in certain positions with respect to it, two or more highly polished reflecting mirrors.

As these conjectures, if well founded, might lead to experiments from the results of which the truth or falsehood of the hypothesis in question might be demonstrated, it is of much importance that this matter should be thoroughly investigated. I shall therefore beg the indulgence of the Society, while I endeavour to examine it with that careful attention which it appears to me to deserve.

When different solid substances, heated to the same degree of temperature, are exposed in the air to cool, those among them which appear to the touch to be the hottest, are not those which cool the fastest, or which send off calorific rays, through the air, in the greatest abundance.

As polished metals reflect a great part of the rays from other bodies which arrive at their surfaces, and as they are neither heated nor cooled by the rays so reflected, their temperatures are slowly changed by the actions of the surrounding bodies at a different temperature.

When a hot polished metallic body is exposed in the air to cool, surrounded by other bodies at the same temperature as that of the cold air, as most of the rays from the surrounding bodies are reflected at the polished surface of the hot body, it is evident that two sorts of rays must proceed from the surface of

that body, namely, those calorific rays which that hot body emits, and those other rays (which with regard to the surrounding bodies are neither calorific nor frigorific) which it reflects.

On a cursory view of the subject, one might be led to imagine, that, as the rays which proceed from the hot metallic body are of two kinds, the energy of the calorific rays, which properly belong to the hot body, might be diminished by those other reflected rays by which they are accompanied, and with which they may be said to be mixed ; but, a more careful examination of the matter will show that this cannot be the case ; that is to say, as long as all the surrounding bodies continue to be at the same temperature. If the temperature of the surrounding bodies be different, such of them will be affected, by the reflected rays, as happen to be of a temperature different from that from which the ray originated ; but still, the effects produced by the rays emitted by the hot body, will be the same, or their power of effecting changes in the temperatures of other (hotter or colder) bodies, will remain undiminished, and unchanged.

The reason why their effects are not more powerful than they are found to be, is not because they are mixed with other reflected rays, but because they are few ; the greater part of the rays which the hot body actually emits being reflected, and turned back upon itself, by the reflecting surface by which it is immediately surrounded.

That the reflecting surface at which the rays of light are turned back and reflected, which impinge against the polished surface of any solid or fluid body, is actually situated *without the body*, and even at some distance from it, has been proved by the most decisive experiments ; and there are so many striking

analogies between the rays of light and those invisible rays which all bodies, at all temperatures, appear to emit, that we can hardly doubt of their motions being regulated by the same laws.

Perhaps there may be no other difference between them, than exists between those vibrations in the air which are audible, and those which make no sensible impression on our organs of hearing.

If the ear were so constructed that we could hear all the motions which take place in the air, we should, no doubt, be stunned with the noise; and, if our eyes were so constructed as to see all the rays which are emitted continually, by day and by night, by the bodies which surround us, we should be dazzled and confounded by that insupportable flood of light, poured in upon us on every side.

Taking it for granted that these invisible radiations exist, we will endeavour to trace the effects which must necessarily be produced by them, in order to see if these investigations will not lead us to a discovery of the causes of some appearances which have hitherto been enveloped in much obscurity.

Suppose two concave reflecting mirrors, of highly polished metal, each 18 inches in diameter, and 18 inches focal distance, to be placed opposite to each other, at the distance of 10 feet, in a large quiet room, in which the air, and the walls of the room, remain constantly at the same temperature, (that of freezing water, for instance,) without any variation.

If we suppose the floor, ceiling, walls of the room, and doors and windows, to be lined with a covering of ice, at the temperature of freezing water, we can then, without any difficulty, conceive that the temperature of the room may remain the same,

notwithstanding the presence of hotter bodies, which are brought into it for the purpose of making experiments.

Let us now suppose one of the mirrors to be at the temperature of freezing, and the other at that of boiling water; and let us see what effects they would produce, on each other, by their radiations.

And first, with respect to the hot mirror, it is evident that it will be cooled, not only by the frigorific rays which proceed from the cold metal of which the opposite mirror is constructed, but also by such of the frigorific rays from the sides of the room as, impinging against the polished reflecting surface of the cold mirror, and being reflected by that surface, happen to fall on the surface of the hot mirror, without being reflected by it.

But, as the quantity of rays which the cold mirror *reflects* is greater, in proportion as the reflecting surface is more perfect, while the quantity of rays emitted by this cold mirror is less, in proportion as its reflecting surface is more perfect, it is extremely probable that the *total* quantity of frigorific rays (emitted and reflected) which, coming from the surface of the cold mirror, impinge against the surface of the hot mirror, will be the same, whatever may be the degree of polish, or reflecting power, of the cold mirror. And, if this be the case, we may conclude, that the presence of this mirror will have no effect whatever on the hot mirror; or, that it will no more expedite its cooling than any other body, of any other form, would do, at the same distance, and occupying the same space.

It might perhaps be imagined, that the *form* of the cold mirror might concentrate the rays it emits and reflects, and, by such concentration, produce a greater effect on the opposite mirror than if its surface were flat, or of any other form; but a more

attentive examination of the matter will show, that no such concentration actually takes place: for, with regard to those rays which are *emitted* by this cold body, as they proceed from each point of its surface *in all directions*, it is perfectly evident that these are not concentrated; and, with respect to those which are *reflected*, it is equally certain that they are not concentrated; because, in order to their being concentrated, they must arrive at the surface of the mirror in parallel lines, and in the direction of the axis of the mirror, which, under the given circumstances, is evidently impossible.

Hence we see, that the presence of the cold mirror will not tend, in the smallest degree, either to accelerate, or to retard, the cooling of the hot mirror; that is to say, provided its temperature be not raised by the calorific rays from the hot mirror.

If its temperature be raised by those rays, it will tend to retard the cooling of the hot mirror; but, even in this case, it will not retard it more than any other polished metallic body would do, of any other form, having the same area, or quantity of surface opposed to the hot mirror, and being placed at the same distance from it.

By a similar train of reasoning, it may be shown, that the *form* of the hot body (that of a concave mirror) will contribute nothing to the effect it will produce on the cold mirror, in heating it, by the calorific rays it emits; and that it will itself be cooled neither faster nor slower, on account of its peculiar form.

Let us now suppose both mirrors to be at the temperature, precisely, of the room, (that of freezing water;) and, that a bullet, or other small body of a spherical form, at the temperature of boiling water, be placed in the focus of one of the mirrors; which mirror we shall call A.

As the rays emitted by this hot body are sent off in right lines, in all directions, in the same manner as light is emitted by luminous bodies, all those rays which fall on the concave polished surface of the mirror A, will be reflected (as is well known) in lines nearly parallel to the axis of the mirror; they will consequently fall on the concave polished surface of the opposite mirror B; and, being there again reflected, they will be *concentrated* at the focus of the second mirror.

If now a sensible thermometer, at the temperature of the room, be placed in this focus, it will immediately begin to rise, in consequence of the heat generated in it by the action of these calorific rays, so accumulated in that place.

If, instead of being placed in the focus of this second mirror, the thermometer be placed at a very small distance from that focus, on one side of it, the instrument, however sensible it may be, will not be apparently affected by the rays from the hot body.

This experiment, which is of ancient date, has often been made, and always with the same results.

Let us now suppose the hot body to be removed from the focus of the mirror A; and that a colder body be substituted in place of it. And, in the first place, we will suppose the temperature of this colder body to be that of freezing water, or just equal to that which reigns in the room.

As the rays which bodies at the same temperature send off from one to the other, have no tendency to increase, or to diminish, the temperature of those bodies, the concentration of rays in the focus of the mirror B, proceeding from the ice-cold body placed in the focus of the mirror A, can have no effect on a

thermometer, at the same temperature, which is exposed to their action.

If heat be a vibratory motion of the constituent particles of bodies, and if the rays which sensible bodies send off in all directions be undulations in an ethereal elastic fluid by which they are surrounded, occasioned by those motions; as the pulsations in this fluid must be isochronous with the vibrations by which they are occasioned, these pulsations or undulations can neither accelerate nor retard the vibrations of other bodies at the surfaces of which they arrive; provided the vibrations of the constituent particles of such bodies are, at that time, isochronous with the vibrations of the constituent particles of the body from which these undulations proceed. But, to return to our experiment.

Suppose now that, instead of this ice-cold body, another much colder, at the temperature of freezing mercury, for instance, be placed in the focus of the mirror A, and that a thermometer at the temperature of freezing water be placed in the focus of the mirror B; what might be expected to be the result of this experiment?—That the thermometer would fall, in consequence of its being cooled by the accumulation of frigorific rays proceeding from this very cold body.

Now this is what actually happened, in the celebrated experiment of my ingenious friend Professor PICTET, of Geneva.

Several attempts have been made to explain the result of that experiment, on the supposition that caloric has a real or material existence, and that radiant heat is that substance, emitted and sent off in right lines, in all directions, from the surfaces of hot bodies. But none of these explanations appear to me to be

satisfactory. One of the most plausible of them, is that which is founded on a supposition that caloric is emitted continually, under the form of radiant heat, by all bodies, at all temperatures; but in greater abundance by hot bodies than by such as are colder; and that a body, at the same time that it sends off radiant caloric in all directions, to the bodies by which it is surrounded, receives it in return, in greater or less quantities, from all those bodies;—that, in all cases where a body, in any given time, receives more radiant caloric than it gives off, an accumulation of caloric in the body takes place, in consequence of which accumulation it becomes hotter;—but, when it gives off more caloric in any given time than it receives, its quantity of caloric is gradually diminished, and it becomes colder;—and, that a constant temperature results, from the quantities of caloric emitted and received continually being equal. But, besides the difficulty of explaining how, or by what mechanism, it can be possible for the same body to receive and retain, and reject and drive away, the same kind of substance, at one and the same time, (an operation not only incomprehensible, but apparently impossible, and to which there is nothing to be found analogous, to render it probable,) many other reasons might be brought to show, that this hypothesis, of the supposed continual interchanges of caloric between neighbouring bodies, is very improbable; and, among the rest, there is one which appears to me to be quite conclusive.

As the point in dispute seems to be of great importance to the science of heat, I shall endeavour to examine it with all possible attention; and, in order to put the hypothesis in question to the test, we will see if it will accord with the results of

some of the foregoing experiments; which, in order to their being more easily comprehended and examined, I shall elucidate by figures.

Let the two opposite ends of the cylinders A and B (Plate V. Fig. 4.) represent the two vertical metallic disks, of equal dimensions, which were presented, at the same time, to the ball of the thermoscope C, in the experiment No. 23.

In that experiment, the disk A being at the temperature of 32° F. (that of freezing water,) and the disk B at 112° F. while the ball of the thermoscope C, and all other surrounding bodies, were at 72° , it was found, that the temperature of the thermoscope was not changed by the simultaneous actions of these two bodies, the one hot, and the other cold.

In order to account for this result, on the hypothesis before mentioned, we must begin by supposing that the ball of the thermoscope gives off radiant caloric continually, in all directions, and receives it, in return, from the surfaces of all the bodies by which it is surrounded.

With regard to all these surrounding bodies, (excepting the disks A and B,) as they are at the same temperature as the ball of the thermoscope, (that of 72° ,) they will give continually to that instrument, just as much radiant caloric as they receive from it; and no change of temperature will result from these equal interchanges.

But, in respect to the disk A, as that is colder than the ball of the thermoscope, it returns to it a smaller quantity of radiant caloric than it receives from it; consequently, the thermoscope receives continually less than it gives: it would of course be gradually exhausted of caloric, and become colder, were it not

for the compensation it receives for this loss, from the disk B. This disk, being hotter than the thermoscope, gives to it continually, more radiant caloric than it receives from it; and, were it not for the simultaneous loss of caloric which the instrument sustains, in its interchanges with the cold disk A, its quantity of caloric would be augmented, and it would become hotter.

Now, as the temperature of the ball of the thermoscope is an arithmetical mean between that of the disk A and that of the disk B, it is reasonable to suppose, that the thermoscope receives just as much more caloric from B than it gives to it, as it gives to A more than it receives from it; and, if that be the case in fact, it is evident, that the simultaneous actions of the two disks on the ball of the thermoscope (or the traffic which they carry on with it in caloric) can neither tend to increase, nor to diminish, the original stock of that substance belonging to that instrument; consequently, the instrument will neither be heated, nor cooled, by these interchanges, but will continue invariably at the same constant temperature.

This explanation is plausible; but, before the hypothesis on which it is founded can be admitted, we must see if it will agree with the results of other experiments; for the greatest care ought always to be used in the admission of hypotheses in physical researches; and, in no case can it be more indispensably necessary, than where an hypothesis has evidently been contrived for the sole purpose of explaining a single experiment, or elucidating a new fact.

When the surface of the metallic disk B was blackened, by holding it over the flame of a candle, the intensity of its radiation,

at the given temperature, (that of 112° ;) was found to be very considerably increased; and when (being so blackened) it was again presented to the ball of the thermoscope, at the same distance as in the last-mentioned experiment, and the cold disk A (at the temperature of 32°) was placed opposite to it, at an equal distance, as represented in Fig. 5, the thermoscope, instead of continuing to retain its original temperature, (that of 72° ;) was now gradually heated.

There is nothing, it is true, in that event, which appears difficult to explain on the assumed principles; for, if the quantity of radiant caloric emitted by the disk B, be increased by blackening its surface, the quantity received from it by the ball of the thermoscope must be increased also; and that additional quantity must of course tend to raise the temperature of the instrument. But here is an experiment which cannot be explained on those principles.

The surface of the cold disk A having been blackened, as well as that of the hot disk B, when both disks (blackened) were again presented, at equal distances, to the ball of the thermoscope, as represented in Fig. 6, it was found, that the original temperature of the thermoscope remained unchanged.

The result of this most interesting experiment proves, that the ball of the thermoscope was just as much cooled by the influence of the cold blackened disk, as it was heated by the hot blackened disk.

Now, as it was found by experiment, that the intensity of the radiation of the disk B was *increased* by the blackening of the surface of that disk, we must conclude, that the intensity of the radiation of the disk A was likewise *increased* by the use

of the same means : but, if those radiations be *caloric*, emitted by those bodies, (which the hypothesis in question supposes,) how did it happen, that the ball of the thermoscope, instead of being *more heated* by the additional quantity of caloric which it received in consequence of the blackening of the disk A, was actually *more cooled*?

It may perhaps be said, by the advocates for the hypothesis in question, that the blackening of the surface of the disk A, caused a greater quantity of caloric to be sent off to it by the ball of the thermoscope. Without insisting on an explanation of the mode of action of the cause which is supposed to produce this effect, (which I might certainly do, as the supposition is perfectly gratuitous,) I will content myself with just observing, that as the surface of the opposite disk *was also blackened*, this supposed augmentation of the quantity of caloric emitted by the ball of the thermoscope, *occasioned by the blackening of the surfaces of the bodies presented to it*, can be of no use in explaining the phenomena in question.

The results of the two last mentioned experiments appear to me to be very important ; and I do not see how they can be reconciled with the opinions of modern chemists, respecting the nature of heat.

In order to simplify our speculations on this abstruse subject, we have hitherto supposed, that *difference of temperature* depends solely on the difference of *the times* of the vibrations of the component particles of bodies. It is possible, however, and even probable, that it depends principally on the *velocities* of those particles : for it is easy to perceive, that the more rapid the motions of those particles are, the greater their elongations must

be, in their vibrations ; and the more, of course, will the volume of the body they compose be expanded.

It is well known, that the pulsations occasioned in an elastic fluid, by the vibrations of an elastic solid body, proceed from that body in all directions ; and that these pulsations are every where (that is to say, at all distances from the body) isochronous with the vibrations of the solid body ; it is known also, that the mean velocity of any individual particle of the fluid is less, in proportion as the distance of the particle is greater from the centre from which these pulsations proceed.

In the case of the pulsations occasioned in the air by the vibrations of sonorous bodies, those pulsations are every where isochronous with the vibrations of the sonorous body ; and the time, or *frequency* of those pulsations, determines the *note* ; but it is the *velocity* of the particles of the air, or the breadth of the wave, on which the *force* or *strength* of the sound depends ; and this velocity becoming less, as the distance from the sonorous body increases, the sound is weakened in the same proportion.

There are several circumstances which might lead us to suspect, that *colour* depends on the *frequency* of those pulsations which have been supposed to constitute light ; and that the *beat* produced by them is in proportion to their *force*.

If this supposition should be well founded, a knowledge of that important fact might perhaps enable us to explain several very interesting phenomena ;—the combustion of inflammable bodies, for instance ; and the great intensity of the heat which is produced by the *concentration* of calorific rays.

There are several well known experiments with burning glasses, which show that the intensity of the heat generated by

the concentration of the solar rays, is not simply as the *condensation* of those rays, but in a higher proportion; and, that it depends much on their *direction*, being greater, as the angle is greater at which they meet at the focus of the lens.

That fact is certainly very remarkable. It has often been the subject of my meditations; and it has contributed not a little to the opinion I have been induced to adopt, respecting the nature of light and of heat. I never could reconcile it with the supposition that heat is caused by the *accumulation* of any thing *emitted* by the sun; or by any other body which sends off calorific radiations.

RESERVING for a future communication, an account of the sequel of my enquiries respecting the subject which I have undertaken to investigate, I shall conclude this long Paper with some observations, concerning the *practical uses* that may be derived from a knowledge of the facts which have been established by the results of the foregoing experiments.

In all cases where it is designed to *preserve the heat* of any substance which is confined in a metallic vessel, it will greatly contribute to that end, if the external surface of the vessel be very clean and bright: but, if the object be to *cool* any thing quickly, in a metallic vessel, the external surface of the vessel should be painted, or covered with some of those substances which have been found to emit calorific rays in great abundance.

Polished tea-urns may be kept boiling hot with a much less expence of spirit of wine (burnt in a lamp under them) than such as are varnished; and the cleaner and brighter the dishes, and covers for dishes, are made, which are used for bringing

victuals on the table, and for keeping it hot, the more effectually will they answer that purpose.

Saucepans, and other kitchen utensils, which are very clean and bright on the outside, may be kept hot with a smaller fire than such as are black and dirty; but the bottom of a saucepan, or boiler, should be blackened, in order that its contents may be made to boil quickly, and with a small expence of fuel.

When kitchen utensils are used over a fire of sea-coal, or of wood, there will be no necessity for blackening their bottoms, for they will soon be made black by the smoke; but, when they are used over a clear fire made with charcoal, it will be adviseable to blacken them; which may be done in a few moments, by holding them over a wood or coal fire, or over the flame of a lamp, or candle.

Proposals have often been made for constructing the broad and shallow vessels (flats) in which brewers cool their wort, of metal; on a supposition that the process of cooling would go on faster in a metallic vessel than in a wooden vessel; but this would not be found to be the case in fact, a metallic surface being ill calculated for expediting the emission of calorific rays.

The great thickness of the timber of which brewers flats are commonly made, is a circumstance very favourable to a speedy cooling of the wort; for, when the flats are empty, this mass of wet wood is much cooled, not only by the cold air which passes over it, but also, and more especially, by evaporation; and, when the flat is again filled with hot wort, a great part of the heat of that liquid is absorbed by the cold wood.

In all cases where metallic tubes filled with steam are used

for warming rooms, or for heating drying-rooms, the external surface of those tubes should be painted, or covered with some substance which facilitates the emission of calorific rays. A covering of thin paper will answer that purpose very well, especially if it be black, and if it be closely and firmly attached to the surface of the metal with glue.

Tubes which are designed for *conveying* hot steam from one place to another, should either be well covered up with *warm* covering, or should be kept clean and bright. It would, I am persuaded, be worth while, in many cases, to gild them, or at least to cover them with what is called gilt paper, or with tin foil, or some other metallic substance which does not easily tarnish in the air.

The cylinders, and principal steam-tubes of steam-engines, might be covered, first with some warm clothing, and then with thin sheet brass, kept clean and bright. The expence of this covering would, I am confident, be amply repaid, by the saving of heat and fuel which would result from it.

If garden walls painted black acquire heat faster, when exposed to the sun's direct rays, than when they are not so painted, they will likewise cool faster, during the night; and gardeners must be best able to determine whether these rapid changes of temperature are, or are not, favourable to fruit trees.

Black clothes are well known to be very warm in the sun; but they are far from being so in the shade, and especially in cold weather. No coloured clothing is so cold as black, when the temperature of the air is below that of the surface of the skin, and when the body is not exposed to the action of calorific rays from other substances.

It has been shown, that the warmth of clothing depends much on the *polish* of the surface of the substance of which it is made; and hence we may conclude that, in choosing the colour of our winter garments, those dyes should be avoided which tend most to destroy that polish: and, as a white surface reflects more light than an equal surface, equally polished, of any other colour, there is much reason to think that white garments are warmer than any other, in cold weather. They are universally considered as the coolest that can be worn, in very hot weather, and especially when a person is exposed to the direct rays of the sun; and, if they are well calculated to reflect calorific rays in summer, they must be equally well calculated to reflect those frigorific rays by which we are cooled and annoyed in winter.

I have found, by direct and decisive experiments, (of which an account will hereafter be given to this Society,) that garments of fur are much warmer, in cold weather, when worn with the fur or hair outwards, than when it is turned inwards. Is not this a proof that we are kept warm by our clothing, not so much by confining our heat, as by keeping off those frigorific rays which tend to cool us?

The fine fur of beasts, being a highly polished substance, is well calculated to reflect those rays which fall on it; and, if the body were kept warm by the rays which proceed from it being reflected back upon it, there is reason to think, that a fur garment would be warmest when worn with the hair inwards; but, if it be by reflecting and turning away the frigorific rays from external (colder) bodies, that we are kept warm by our clothes in cold weather, we might naturally expect, that

a pelisse would be warmest when worn with the hair outwards, as I have found it to be in fact.

The point here in question is by no means a matter of small importance; for, until the principles of the warmth of clothing be understood, we shall not be able to take our measures with certainty, and with the least possible trouble and expence, for defending ourselves against the inclemencies of the seasons, and making ourselves comfortable in all climates.

The fur of several delicate animals becomes white in winter, in cold countries; and that of the bears which inhabit the polar regions, is white in all seasons. These last are exposed alternately, in the open air, to the most intense cold, and to the continual action of the sun's direct rays during several months. If it should be true that heat, and cold, are excited in the manner above described, and that white is the colour most favourable to the reflection of calorific and frigorific rays, it must be acknowledged, even by the most determined sceptic, that these animals have been exceedingly fortunate, in obtaining clothing so well adapted to their local circumstances.

The excessive cold which is known to reign, in all seasons, on the tops of very high mountains, and in the higher regions of the atmosphere, and the frosts at night, which so frequently take place on the surface of the plains below, in very clear and still weather, in spring and autumn, seem to indicate, that frigorific rays arrive continually at the surface of the earth, from every part of the heavens.

May it not be by the action of these rays that our planet is cooled continually, and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities

of heat that are generated at its surface, by the continual action of the solar rays?

If this conjecture should be well founded, we should be led to conclude, that the inhabitants of certain hot countries, who sleep at night on the tops of their houses, in order to be more cool and comfortable, do wisely, in choosing that situation to pass their hours of rest.

Fig. 1.

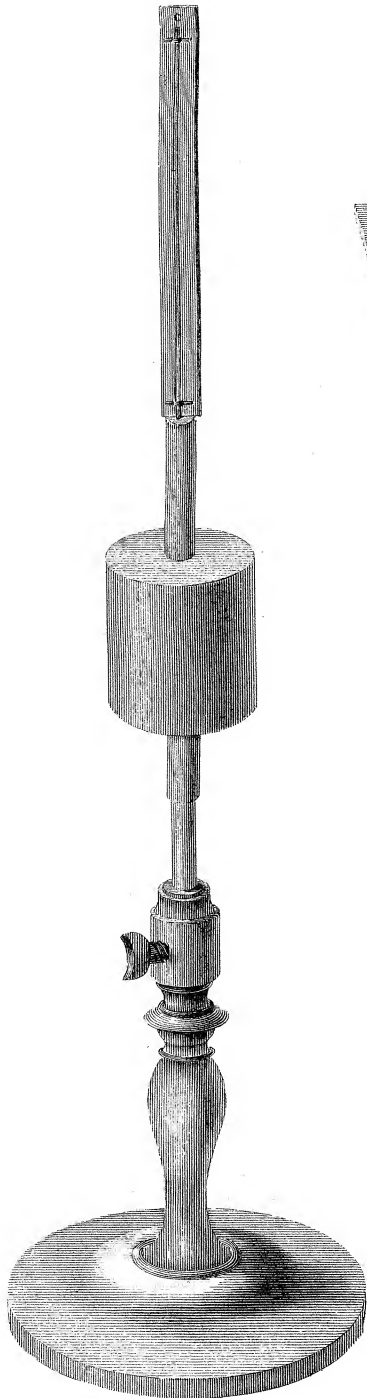


Fig. 2.

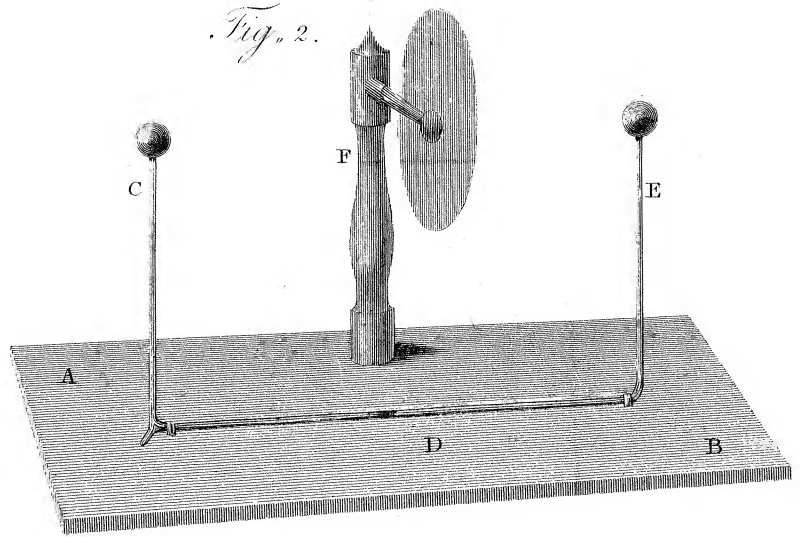


Fig. 3.

